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PROCEEDINGS

OF

THE AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.

EIGHTH MEETING,

HELD AT WASHINGTON, D. C.,

MAY, 1854.

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John P. Foote, Esq., Cincinnati.
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Committee to Revise the Constitution.

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Prof. J. Lawrence Smith,
Prof. John Leconte,
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Dr. B. A. Gould,
Prof. W. B. Rogers,
Prof. J. D. Dana,
Dr. Joseph Leidy,
Prof. S. S. Haldeman,
Dr. A. A. Gould.

Committee to Memorialize Congress in Relation to a Geographical Department of the Congress Library.

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Gen. Jos. G. Totten, U. S. A.,
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Prof. Arnold Guyot,
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Prof. S. St. John,
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Prof. A. D. Bache,
Prof. W. B. Rogers,
Prof. Elias Loomis,
Lieut. M. F. Maury, U. S. N.,
Prof. Joseph Henry.

B. New Committees.

Committee to take Proper Measures in Regard to the Annular Eclipse of May 26th, 1854.

Prof. A. D. Bache,
Prof. Benjamin Peirce,
Prof. Stephen Alexander,
Capt. C. H. Davis,
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George P. Bond,
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Committee to Report in Relation to Uniform Standards in Weights, Measures, and Coinage.

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Prof. J. H. Alexander,
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Committee to Audit the Accounts of the Treasurer.

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William S. Vaux, Esq.

Consulting Committee on the Publication of the Washington Proceedings.

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Dr. A. L. Elwyn, *Treasurer.*

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George Thurber, Esq.,
Prof. George J. Chase,
Stephen Olney, Esq.,
Hon. Thomas P. Shepard.

MEETINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

	Place.	Date.	President.	General Secretary.	Permanent Sec'y.	Treasurer.
1st Meeting,	Philadelphia, Pa.	September 20, 1848,	W. C. Redfield, Esq.,	Prof. Walter R. Johnson,	. . .	Prof. J. Wyman.
2d "	Cambridge, Mass.	August 14, 1849,	Prof. Joseph Henry,	Prof. E. N. Horsford,	. . .	Dr. A. L. Elwyn.
3d "	Charleston, S. C.	March 12, 1850,	Prof. A. D. Bache,*	Prof. L. R. Gibbes,*	. . .	Dr. St. J. Ravenel.*
4th "	New Haven, Ct.	August 19, 1850,	Prof. A. D. Bache,	E. C. Herrick, Esq.,	. . .	Dr. A. L. Elwyn.
5th "	Cincinnati, Ohio,	May 5, 1851,	Prof. A. D. Bache,	Prof. W. B. Rogers,	Prof. S. F. Baird,	Dr. A. L. Elwyn.
6th "	Albany, N. Y.	August 19, 1851,	Prof. L. Agassiz,	Prof. W. B. Rogers,	Prof. S. F. Baird,	Dr. A. L. Elwyn.
7th "	Cleveland, Ohio,	July 28, 1853,	Prof. Benj. Peirce,	Prof. J. D. Dana,	Prof. S. F. Baird,	Dr. A. L. Elwyn.
8th "	Washington, D.C.	April 26, 1854,	Prof. J. D. Dana,	Prof. J. Lawrence Smith,	Prof. J. Lovering.	Dr. J. L. Le Conte.*

* In the absence of the regular officer.

CONSTITUTION OF THE ASSOCIATION.

OBJECTS.

THE Society shall be called "The American Association for the Advancement of Science." The objects of the Association are, by periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of the United States; to give a stronger and more general impulse, and a more systematic direction, to scientific research in our country; and to procure for the labors of scientific men increased facilities and a wider usefulness.

RULES.

MEMBERS.

RULE 1. Those persons whose names have been already enrolled in the published proceedings of the Association, and all those who have been invited to attend the meetings, shall be considered members, on subscribing to these rules.

RULE 2. Members of scientific societies, or learned bodies, having in view any of the objects of this Society, and publishing transactions, shall likewise be considered members, on subscribing to these rules.

RULE 3. Collegiate professors of natural history, physics, chemistry, mathematics, and political economy, and of the theoretical and applied sciences generally, also civil engineers and architects who have been employed in the construction or superintendence of public works, may become members, on subscribing to these rules.

RULE 4. Persons not embraced in the above provisions may become members of the Association, upon nomination by the Standing Committee, and by a majority of the members present.

OFFICERS.

RULE 5. The officers of the Association shall be a President, a Secretary, and a Treasurer; who shall be elected at each annual meeting, for the meeting of the ensuing year.

MEETINGS.

Rule 6. The Association shall meet annually, for one week or longer, the time and place of each meeting being determined by a vote of the Association at the previous meeting; and the arrangements for it shall be intrusted to the officers and the Local Committee.

STANDING COMMITTEE.

Rule 7. There shall be a Standing Committee, to consist of the President, Secretary, and Treasurer of the Association; the officers of the preceding year; the chairmen and secretaries of the Sections, after these shall have been organized; and six other members present, who shall have attended any of the previous meetings: to be elected by ballot.

Rule 8. The Committee, whose duty it shall be to manage the general business of the Association, shall sit during the meeting, and at any time in the interval between it and the next meeting, as the interests of the Association may require. It shall also be the duty of this Committee to nominate the general officers of the Association for the following year, and persons for admission to membership.

SECTIONS.

Rule 9. The Standing Committee shall organize the Society into Sections, permitting the number and scope of these Sections to vary in conformity to the wishes and the scientific business of the Association.

Rule 10. It shall be the duty of the Standing Committee, if, at any time, two or more Sections, induced by a deficiency of scientific communications, or by other reasons, request to be united into one, — or if at any time a single Section, overloaded with business, asks to be subdivided, — to effect the change, and generally to readjust the subdivisions of the Association, whenever, upon due representation, it promises to expedite the proceedings, and advance the purposes of the meeting.

SECTIONAL COMMITTEES AND OFFICERS.

Rule 11. Each Section shall appoint its own chairman and secretary of the meeting; and it shall likewise have a standing committee, of such size as the Section may prefer. The secretaries of the Sections may appoint assistants, whenever, in the discharge of their duties, it becomes expedient.

Rule 12. It shall be the duty of the standing committee of each Section, assisted by the chairman, to arrange and direct the proceedings in their Section, to ascertain what written and oral communications are offered, and, for the better forwarding the business, to assign the order in which these communications shall appear, and the amount of time which each shall occupy; and it shall be the duty of the chairman to enforce these decisions of the committee.

These Sectional Committees shall likewise recommend subjects for systematic investigation, by members willing to undertake the researches, and present their results at the next annual meeting.

The committees shall likewise recommend reports on particular topics and departments of science, to be drawn up as occasion permits, by competent persons, and presented at subsequent annual meetings.

REPORTS OF PROCEEDINGS.

RULE 13. Whenever practicable, the proceedings shall be reported by professional reporters or stenographers, whose reports are to be revised by the secretaries before they appear in print.

PAPERS AND COMMUNICATIONS.

RULE 14. The author of any paper or communication shall be at liberty to retain his right of property therein, provided he declares such to be his wish before presenting it to the Society.

GENERAL AND EVENING MEETINGS.

RULE 15. At least three evenings of the week shall be reserved for general meetings of the Association, and the Standing Committee shall appoint these and any other general meetings which the objects and interests of the Association may call for.

These general meetings may, when convened for that purpose, give their attention to any topics of science which would otherwise come before the Section; and thus all the Sections may, for a longer or shorter time, reunite themselves to hear and consider any communications, or transact any business.

It shall be a part of the business of these general meetings to receive the Address of the President of the last Annual Meeting; to hear such reports on scientific subjects as, from their general importance and interest, the Standing Committee shall select; also to receive from the chairmen of the Sections abstracts of the proceedings of their respective Sections; and to listen to communications and lectures explanatory of new and important discoveries and researches in science, and new inventions and processes in the arts.

ORDER OF PROCEEDINGS IN ORGANIZING A MEETING.

RULE 16. The Association shall be organized by the President of the preceding Annual Meeting. The question of the most eligible distribution of the Society into Sections shall then occupy the attention of the Association; when, a sufficient expression of opinion being procured, the meeting may adjourn; and the Standing Committee shall immediately proceed to divide the Association into Sections, and to allot to the Sections their general places of meeting. The Sections may then organize by electing their officers, and proceed to transact scientific and other business.

LOCAL COMMITTEE.

RULE 17. The Standing Committee shall appoint a Local Committee from among members residing at or near the place of meeting for the ensuing year; and it shall be the duty of the Local Committee, assisted by the officers, to make arrangements for the meeting.

SUBSCRIPTIONS.

RULE 18. The amount of the annual subscription of each member of the Association shall be two dollars; and one dollar in addition shall entitle him to a copy of the proceedings of the annual meeting. The members attending an annual meeting shall pay, on registering their names, an additional assessment of ——— dollars. These subscriptions to be received by the Treasurer or Secretary.

RULE 19. The names of all persons two years and more in arrears for annual dues, shall be erased from the list of members; provided that two notices of indebtedness, at an interval of at least three months, shall have previously been given.

ACCOUNTS.

RULE 20. The accounts of the Association shall be audited annually, by auditors appointed at each meeting.

ALTERATIONS OF THE CONSTITUTION.

RULE 21. No article of this Constitution shall be altered or amended, without the concurrence of three fourths of the members present, nor unless notice of the proposed amendment or alteration shall have been given at the preceding annual meeting.

RESOLUTIONS AND ENACTMENTS

OF A PERMANENT AND PROSPECTIVE CHARACTER,

PASSED PREVIOUS TO THE EIGHTH MEETING.

Resolved, That copies or abstracts of all communications made, either to the General Association or to the Sections, must be furnished by the authors; otherwise, only the titles shall appear in the published proceedings. (*Proceedings First Meeting*, 1848, p. 133.)

Resolved, That 1,000 copies of the Proceedings of the Association be published in pamphlet form, and placed at the disposal of the chairman of the Committee on Publication.

(*Proceedings First Meeting*, 1848, p. 133.)

Resolved, That a *manual* or *manuals of scientific observation and research*, especially adapted to the use of the American inquirer, comprising directions for properly observing phenomena in every department of physical science, and for making collections in natural history, etc., whether on land or at sea, is much needed at the present time; and that such a publication, placed in the hands of officers of the army and navy, would greatly tend to develop the natural resources of our extended country, and to the general advancement of science.

Resolved, That the American Association for the Advancement of Science cordially recommends the Smithsonian Institution to undertake the preparation of such a volume, under the editorial superintendence of its Secretary, to be published in its series of reports.

Resolved, That this Association will cordially coöperate in the production of such a manual or manuals, in whatever manner may be best adapted to secure the end in view.

(*Proceedings Second Meeting*, 1849, pp. 273, 351.)

Resolved, That no paper be read before the future meetings of the Association, unless an abstract of it has previously been presented to the Secretary.

Resolved, That hereafter all books, charts, maps, and specimens, which may be presented to the Association, shall be given to the Smithsonian Institution.

(*Proceedings Second Meeting*, 1849, p. 272.)

Resolved, That a Secretary be appointed, who shall hold his office for the term of three years, and shall have a salary of $300 per annum, and whose duty it shall be to compile for publication all proceedings or transactions of the Association, to superintend the publication of the same, and to conduct the correspondence; the title of said officer to be that of Permanent Secretary of the American Association. (*Proceedings Fourth Meeting*, 1850, p. 16.)

Resolved, That the Standing Committee have power to fix the duties of the Permanent Secretary of this Association.

Resolved, That the Permanent Secretary be a member, *ex officio*, of the Standing Committee.

Resolved, That the Permanent Secretary be instructed to erase from the list of members of this Association, the names of all who, by the return of the Treasurer, shall appear to be two years in arrears for annual dues; suitable notice being given by two circulars from the Treasurer, at an interval of three months, to all who may fall within the intent of this resolution.

Resolved, That the Standing Committee have full power to complete and finish any outstanding business of the Association, in their name. (*Proceedings Fourth Meeting*, 1850, p. 341.)

Resolved, That a copy of the printed volume of Proceedings of the Meetings at Philadelphia, Cambridge, and New Haven, be presented to the libraries of Harvard and Yale.

(*Proceedings Fourth Meeting*, 1850, p. 346.)

Resolved, That the Treasurer be requested to retain $300 of the funds in his hands, and belonging to the Association, for the purpose of paying the salary of the Permanent Secretary; said payment to be made at such time, and in such manner, as may be agreed upon by the Treasurer and Permanent Secretary.

Resolved, That copies of the Proceedings of the American Association be presented to the New York Lyceum and the Philadelphia Academy of Natural Sciences.

Resolved, That the Permanent Secretary be requested to provide minute-books, suitably ruled, for the list of members and titles of papers, minutes of the general and sectional meetings, and for the other purposes indicated in the rules.

Resolved, That whenever the Permanent Secretary notices any error of fact or unnecessary repetition, or any other important defect in the papers communicated for publication in the Proceedings of the Association, that he be authorized to commit the same to the author, or to the proper sub-committee of the Standing Committee, for correction. (*Proceedings Fourth Meeting*, 1850, pp. 390, 391.)

Resolved, That copies of the Proceedings of the American Association for the Advancement of Science be presented to the American Academy of Arts and Sciences, Boston; to the Boston Society of Natural History; to the New York Lyceum of Natural History; to the American Philosophical Society and to the Academy of Natural Sciences of Philadelphia; to the Smithsonian Institution; and to the Western Academy of Natural Sciences at Cincinnati.

(*Proceedings Fifth Meeting*, 1851, p. 249.)

Resolved, That the Standing Committees of the Sections be requested, before the close of the meeting, to present to the Permanent Secretary a list of the papers which have been read in the Sections, and which they desire to have published.

Resolved, That hereafter all members of this Association are particularly desired to forward to the Permanent Secretary, so as to be received before the day appointed for the Association to convene, complete titles of all the papers which they expect to present during its meeting, with an estimate of the time required for reading each, and such abstracts of their contents as may give a general idea of their nature.

Resolved, That the foregoing resolution form part of the Circular.

(*Proceedings Sixth Meeting*, 1852, p. 402.)

Resolved, That the annual fee of membership be $ 2.00 ; and that payment of an additional dollar entitle a member to a copy of the Proceedings of the Meeting.

Resolved, That all members of the Association who have not paid their dues, after the issue of two circulars at intervals of three months, notifying them of that fact, be stricken from the roll by the Permanent Secretary.

(*Proceedings Sixth Meeting*, 1852, p. 402.)

Whereas the provision of the Constitution appears to be indefinite in regard to the term of service of the chairmen and secretaries of the Sections, —

Resolved, That the Sections be requested to direct the chairmen of their several Standing Committees to attend to the current business of the Section, and to appoint a chairman for each day of the meeting.

Resolved, That the Sections be requested to appoint a secretary for the period of the meeting, whose duty it shall be to furnish to the Permanent Secretary, for publication, a full report of all proceedings and discussions.

Resolved, That the following resolutions be presented to the Association at the opening of the Cleveland Meeting, for adoption : —

" 1. That all papers, either at the general or in the several sectional meetings, shall be read in the order in which they are entered upon the books of the Association ; except that those which may be entered by a member of the Standing Committee of the Association shall be liable to postponement by the Standing Committees of the Sections."

If this regulation should be adopted by the Association, members will recognize the expediency of entering the titles of their communications at as early a date as possible.

" 2. That, if any communication should not be ready at the assigned time, it shall be dropped to the bottom of the list, and shall not be entitled to take precedence of any subsequent communication.

" 3. That no exchanges shall be made between members, without authority of the respective Standing Committees."

(*Proceedings Sixth Meeting*, 1852, p. 405.)

Resolved, That the names of those only shall be entered in the list of members who shall have signified their acceptance.

(*Proceedings Seventh Meeting*, 1853.)

Resolved, That a sum not exceeding seventy-five dollars shall be paid to the Permanent Secretary, to defray the expenses necessary for attending each meeting of the Association.

(*Proceedings Seventh Meeting*, 1853.)

1st. *Resolved*, That priority of entry of a paper shall, as far as practicable, give precedence in its presentation; cases of exception to be decided by the Standing Committees of the Sections.

2d. *Resolved*, That, if any communication be not ready at the assigned time, it shall be dropped to the bottom of the list, and shall not be entitled to take precedence of any subsequent communication.

3d. *Resolved*, That no exchange shall be made between members without authority of the respective Standing Committees.

(*Proceedings Seventh Meeting*, 1853.)

Resolved, That the following members be requested to report on the subjects respectively assigned to them, viz.: —

PROF. A. D. BACHE. *On Recent Additions to our Knowledge of the Theory of Tides.*

PROF. JOSEPH HENRY. *On Recent Additions to our Knowledge of the Laws of Atmospheric Electricity.*

PROF. JAMES HALL. *On Recent Additions to our Knowledge of Paleozoic Rocks.*

PROF. J. L. SMITH. *On the Recent Progress of Micro-Chemistry.*

PROF. WOLCOTT GIBBS. *On the Recent Progress of Organic Chemistry.*

DR. JOSEPH LEIDY. *On the Remains of Fossil Reptiles and Mammals in North America.*

PROF. BENJAMIN PEIRCE. *On the Present State of the Theory of Planetary Perturbations.*

DR. W. I. BURNETT. *On Recent Advances in Anatomy and Physiology.*

PROF. LOUIS AGASSIZ. *On the History of our Knowledge of Alternation of Generation in Animals.*

PROF. J. D. DANA. *On the Geographical Distribution of the Lower Animals.*

PROF. S. S. HALDEMAN. *On the Present State of our Knowledge of Linguistic Ethnology.*

DR. B. A. GOULD. *On the Progress and Developments of the Electro-Chronographic Method of Observation.*

(*Proceedings Seventh Meeting*, 1853.)

MEMBERS

OF THE

AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.

NOTE. — Names of deceased members are marked with an asterisk (*); and those of members who, in 1840, formed the original "Association of American Geologists," are in small capitals. The figure at the end of each name refers to the meeting at which the election took place.

A.

Abbott, Gorham D., New York [7].
Abbott, Dr. S. L., Boston [1].
Abert, Col. J. J., Washington, D. C. [1].
*Adams, Prof. C. B., Amherst, Massachusetts [1].
Adams, Solomon, Esq., New York [1].
Adamson, Rev. Wm., England [7].
Agassiz, Prof. Louis, Cambridge, Massachusetts [1].
Aiken, Hon. William, Charleston [3].
Alexander, Dr. R. C., Bath, England [2].
Alexander, Prof. Stephen, Princeton, New Jersey [1].
Alger, Francis, Esq., Boston [1].
Allen, Prof. E. A. H., Troy, New York [6].
Allen, George N., Oberlin, Ohio [5].
Allen, J. Burnet, Esq., Springfield, Massachusetts [2].
Allen, John H., Oxford, Maryland [6].
Allen, Z., Esq., Providence, Rhode Island [1].
Allston, R. F. W., Esq., Georgetown, South Carolina [3].

*Ames, M. P., Esq., Springfield, Massachusetts [1].
Amory, Jonathan, Jamaica Plains, Massachusetts [8].
Anderson, Dr. Henry J., New York [4].
Andrews, Alonzo, Boston [7].
Andrews, E., M. D., Ann Arbor, Michigan [7].
Andrews, Prof. E. B., Marietta, Ohio [7].
Andrews, Prof. J. W., Marietta, Ohio [5].
Andrews, Dr., Charlotte, North Carolina [3].
Andrews, Joel W., Esq., Albany [6].
Anthony, Charles H., Esq., Albany [6].
Anthony, J. G., Esq., Cincinnati, Ohio [1].
Antisell, Dr. Thomas, New York [2].
Appleton, Nathan, Esq., Boston [1].
Appleton, Thomas G., Boston [8].
Arden, Thomas B., Garrison's P. O., Putnam Co., New York [7].
Armsby, Prof. James H., Albany [6].
Ashmead, Samuel, Esq., Philadelphia [1].
Astrop, R. F., Crichton's Store, Burns Co., Virginia [7].
Atkinson, T. C., Alexandria, Virginia [1].
Avery, Prof. Charles, Schenectady, New York [6].
Ayres, William O., Esq., San Francisco, California [1].

B.

Bache, Prof. Alexander D., Washington, D. C. [1].
Bache, Dr. Franklin, Philadelphia [1].
Bachman, Dr. John, Charleston [1].
Bacon, J. S., Pres., Washington, D. C. [1].
Bacon, Dr. John, Jr., Boston [1].
Bacon, William, Richmond, Berkshire Co., Massachusetts [7].
Baer, Prof. William, Sykesville, Maryland [8].
Bagg, Moses M., Clinton, New York [4].
Bailey, Prof. J. W., West Point, New York [1].
Baird, Rev. E. Thompson, Washington College, E. Tenn. [6].
Baird, Prof. S. F., Washington, D. C. [1].
Baird, Thomas D., Esq., Baltimore [6].
Baird, William M., Reading, Pennsylvania [7].
Bakewell, Robert, Esq., New Haven, Connecticut [1].
Baldwin, William, Platte City, Missouri [7].

Bannister, Rev. Henry, Cazenovia, New York [6].
Barbee, Dr. W. T., Jackson, Mississippi [5].
Barker, Dr. Sanford, Charleston [2].
Barlow, Thomas, Canastota, New York [7].
Barnard, Henry, Esq., New Haven, Connecticut [4].
Barnard, F. A. P., Tuscaloosa, Alabama [7].
Barnes, Capt. James, Springfield, Massachusetts [5].
Barratt, Dr. J. P., Barrattsville, South Carolina [3].
Barrows, George B., Fryeburg, Maine [7].
Bartlett, J. R., Providence, Rhode Island [8].
Barton, William, Esq., Troy, New York [6].
Bassnett, Thomas, Ottawa, Illinois [8].
Batchelder, J. M., Cambridge, Massachusetts [8].
Beadle, Dr. Edward L., New York [1].
Beardsley, R. G., Esq., Albany, New York [6].
Beck, Dr. C. F., Philadelphia [1].
*BECK, Prof. LEWIS C., New Brunswick, New Jersey [1].
Beck, Dr. T. Romeyn, Albany, New York [1].
Belknap, George, Esq., Boston [1].
Belknap, Henry, Esq., Boston [2].
Bell, John G., New York City [7].
Bell, John J., Carmel, Penobscot Co., Maine [7].
Bell, Samuel N., Manchester, New Hampshire [7].
Bellows, Rev. H. W., New York [5].
Beman, Rev. Dr., Troy, New York [6].
Benedict, Prof. Farrand N., Burlington, Vermont [1].
Berthoud, Edward L., Maysville, Mason Co., Kentucky [7].
Bigelow, Dr. Henry J., Boston [1].
Bigelow, Dr. Jacob, Boston [2].
Bingham, Rev. J. F., New York [7].
*Binney, Dr. Amos, Boston [1].
Binney, John, Esq., Boston [3].
Blake, Eli W., Esq., New Haven, Connecticut [1].
Blake, William P., Esq., New York [2].
*Blanding, Dr. William, Rhode Island [1].
Blaney, Prof. James V. Z., M. D., Chicago, Illinois [5].
Blasius, Wilhelm, New York [7].
Blatchford, Rev. Thomas W., Troy, New York [6].

Bliss, George, Jr., Esq., Springfield, Massachusetts [3].
Blodget, Lorin, Washington, D. C. [7].
Blumenthal, Prof. C. E., Carlisle, Pennsylvania [5].
Booth, Prof. James C., Esq., Philadelphia [1].
Bolton, Richard, Esq., Pontotoc, Mississippi [1].
*Bomford, Col. George, Washington, D. C. [1].
Bond, George P., Esq., Cambridge, Massachusetts [2].
Bond, Dr. Henry, Philadelphia [1].
Bond, William C., Esq., Cambridge, Massachusetts [2].
Bonnycastle, Sir Charles, Montreal, Canada [1].
Boutelle, C. O., Esq., Coast Survey [3].
Bouve, Thomas T., Esq., Boston [1].
Bowditch, Henry J., M. D., Boston [2].
Bowman, Capt. A. H., U. S. A., Charleston [2].
Boyd, Dr. George W., Charlottesville, Virginia [1].
Boyden, Uriah A., Esq., Boston [2].
Boynton, John F., Esq., Syracuse, New York [4].
Brace, J. P., Hartford, Connecticut [2].
Bradford, Hezekiah, New York [7].
Bradley, Francis G., Esq., New Haven, Connecticut [4].
Brainerd, Prof. Jehu, Cleveland, Ohio [5].
Braw, J. B., Esq., Hartford, Connecticut [5].
Breckenridge, Rev. Dr. Robert T., Lexington, Kentucky [5].
Brevoort, J. Carson, Esq., Brooklyn, New York [1].
Brewer, W. H., Esq., Ithaca, New York [4].
Briggs, C., Esq., Ironton, Ohio [6].
Brinsmade, Dr. Thomas C., Troy, New York [6].
Britton, A. A., Keokuk, Iowa [7].
Brocklesby, Prof. John, Hartford, Connecticut [4].
Bross, William, Chicago, Illinois [7].
Brown, Andrew, Esq., Natchez, Mississippi [1].
Brown, Richard, Esq., Sydney, Cape Breton [1].
Brown, Prof. W. Leroy, Oakland, Mississippi [7].
Browne, Peter A., Esq., Philadelphia [1].
Brownne, Robert H., Esq., New York [1].
Brumby, Prof. Richard T., Tuscaloosa, Alabama [1].
Brush, George J., New Haven, Connecticut [4].
Buchanan, Dr. Joseph R., Cincinnati, Ohio [1].

Buchanan, Robert, Esq., Cincinnati, Ohio [2].
Buckland, David, Brandon, Vermont [7].
Buckley, S. B., Esq., New York [1].
Buell, David, Jr., Troy, New York [6].
Bunce, John B., Esq., New Haven, Connecticut [4].
Burden, Dr. T. L. Charleston [3].
Burgess, Rev. E. Ahmednuggur, India [1].
Burnet, Hon. Jacob, Cincinnati, Ohio [4].
Burnett, Waldo I., Esq., Boston [1].
Burr, E. T., Esq., Lyme, Connecticut [6].
Burton, Dr. C. V. W., Lansingburg, New York [6].
Burwell, Dr. George N., Buffalo, New York [6].

C.

Cabell, Prof. James L., University of Virginia [6].
Cabot, Edward C., Esq., Boston [1].
Cain, Dr. D. J. C., Charleston [3].
Caldwell, John D., Esq., Cincinnati, Ohio [5].
Campbell, Robert, Esq., Pittsfield, Massachusetts [1].
Cappy, Dr. J. P., Montgomery, Alabama [4].
Carey, H. C., Burlington, New Jersey [8].
Carleton, James Henry, Fort Leavenworth, Missouri [7].
Carley, S. T., Esq., Cincinnati, Ohio [5].
Carpenter, George W., Esq., Albany, New York [6].
Carpenter, Thornton, Camden, South Carolina [7].
*Carpenter, Dr. William M., New Orleans [1].
Carter, R. C., Esq., Cincinnati, Ohio [5].
Case, Hon. William, Cleveland, Ohio [6].
Casseday, L. Addison, Esq., Louisville, Kentucky [5].
Cassels, Prof. J. Long, Cleveland, Ohio [7].
Cassin, John, Philadelphia [7].
Caswell, Prof. Alexis, Providence, Rhode Island [2].
Chandler, M. W. T., Philadelphia [8].
Channing, William F., Esq., Boston [2].
*Chapman, Dr. N., Philadelphia [1].
Chappellsmith, John, New Harmony, Indiana [7].
Chase, Rev. Benj., Natchez, Mississippi [7].
Chase, Prof. George J., Providence, Rhode Island [1].

Chase, Theodore R., Cleveland, Ohio [7].
*Chase, Prof. S., Dartmouth, New Hampshire [2].
Chauvenet, C. U., Esq., Annapolis, Maryland [2].
Chauvenet, Prof. William, Annapolis, Maryland [1].
Cheeseborough, A., Esq., Springfield, Massachusetts [5].
Chenoweth, B. D., Esq., Winchester, Virginia [4].
Chesbrough, E. S., West Newton, Massachusetts [2].
Chester, Prof. C., Rochester, New York [8].
Cheves, Dr. C. M., Charleston [3].
Choate, Charles Francis, Cambridge, Massachusetts [7].
Christy, David, Esq., Oxford, Ohio [5].
Clapp, Dr. Asahel, New Albany, Indiana [1].
Clark, Dr. Alonzo, New York [6].
Clark, Alvin, Esq., Cambridge, Massachusetts [4].
Clark, Joseph, Esq., Cincinnati, Ohio [5].
Clark, L. M., Esq., Canandaigua, New York [6].
Clark, Maj. M. Lewis, St. Louis, Missouri [5].
Clark, Prof. P. P., Georgetown, D. C. [8].
Clark, Wm. P., Norwalk, Ohio [7].
Clarke, Lieut. F. M., West Point, New York [1].
Clarke, Prof. F. N., West Point, New York [4].
Clarke, Robert, Cincinnati, Ohio [7].
Clarke, Prof. W. W., Albany, New York [4].
Clay, Joseph A., Esq., Philadelphia [1].
*Cleveland, Dr. A. B., Cambridge, Massachusetts [2].
Cleveland, Prof. Parker, Brunswick, Maine [1].
Clingman, Hon. T. L., Asheville, North Carolina [1].
Coaklay, Prof. George W., St. James's College, Hagerstown, Maryland [5].
Coan, Rev. Titus, Hilo, Hawaii [1].
Coates, Dr. B. H., Philadelphia [1].
Coates, Dr. Reynell, Philadelphia [1].
Coffin, Prof. James H., Easton, Pennsylvania [1].
Coffin, Prof. James H. C., Washington, D. C. [1].
Cogswell, Dr. Mason F., Albany, New York [4].
Cohen, Dr. J. J., Baltimore [1].
*Cole, Thomas, Esq., Salem, Massachusetts [1].
*Coleman, Rev. Henry, Boston [1].

Colton, Willis S., New Haven, Connecticut [8].
Comstock, J. L., Esq., Hartford, Connecticut [5].
Conant, Marshall, South Bridgewater, Massachusetts [7].
Congdon, Charles, Esq., New York [1].
Conkey, Dr. J. S., Antwerp, Jefferson Co., New York [6].
Convers, Charles C., Esq., Zanesville, Ohio [6].
Cook, Prof. George H., Albany, New York [6].
Cooke, Robert L., Bloomfield, Essex Co., New Jersey [7].
Cooke, George H., Esq., Troy, New York [2].
Cooke, Josiah P., Esq., Cambridge, Massachusetts [2].
Cooper, William, Esq., New York [1].
Corning, Hon. Erastus, Albany, New York [6].
Cotting, Dr. B. E., ———, Georgia [1].
Couch, Lieut. Darius N., Washington, D. C. [8].
Couper, J. Hamilton, Esq., Darien, Georgia [1].
Couthouy, Joseph P., Esq., New York [1].
Cozzens, Issachar, Esq., New York [1].
Craw, William J., Esq., New Haven, Connecticut [4].
Cresson, J. C., Esq., Philadelphia [2].
Croswell, Edwin, Esq., Albany, New York [6].
Culman, R., Bavaria [4].
Curley, Prof. James, Georgetown, D. C. [8].
Curtis, James L., Esq., New York [6].
Curtis, Dr. Josiah, Boston [1].
Curtis, L. W., Unionville, South Carolina [2].
Cutts, Richard D., San Francisco, California [7].

D.

Dahlgren, J. A., U. S. N., Washington, D. C. [7].
Dalson, Lieut. Augustus, Cambridge, Massachusetts [6].
Dana, Prof. James D., New Haven, Connecticut [1].
Daniels, Edward, Ceresco, Wisconsin [7].
Daniels, Dr. W. C., Savannah, Georgia [1].
Darby, John, Esq., Culloden, Georgia [5].
Dascomb, Prof. James, Oberlin, Ohio [7].
Davenport, Rev. James R., Albany, New York [6].
Davis, Com. C. H., U. S. N., Cambridge, Massachusetts [1].
Davis, Dr. Charles, Charleston [3].

Davis, Rev. Emerson, Westfield, Massachusetts [6].
Davis, James, Jr., Esq., Boston [1].
Day, Dr. Jeremiah, New Haven, Connecticut [4].
Dayton, A. O., Esq., Washington, D. C. [4].
Dayton, Edwin A., Madrid, St. Lawrence Co., New York [7].
Dean, Prof. Amos, Albany, New York [6].
Dean, Philotus, Allegheny City, Pennsylvania [7].
*Dearborn, Gen. H. A. S., Roxbury, Massachusetts [1].
Dearborn, William L., Esq., Roxbury, Massachusetts [1].
*DeKay, Dr. James E., New York [1].
Delafield, Joseph, Esq., New York [1].
Delano, Joseph C., Esq., New Bedford, Massachusetts [5].
Desor, E., Esq., Cambridge, Massachusetts [1].
DeSaussure, H. A., Esq., Charleston [3].
DeSaussure, Dr. H. W., Charleston [3].
Dewey, Prof. Chester, Rochester, New York [1].
Dexter, James, Esq., Albany, New York [6].
Dilke, C. Wentworth, London, England [7].
Dinwiddie, Robert, Esq., New York [1].
Dixwell, Epes S., Esq., Cambridge, Massachusetts [1].
Dod, Prof. William, Covington, Kentucky [5].
D'Orbigny, M. Alcide, Paris, France [1].
Douglass, C. C., Esq., Detroit, Michigan [1].
Douglass, Prof. Silas H., Ann Arbor, Michigan [6].
Drown, Prof. Charles, Troy, New York [6].
*Ducatel, Dr. J. T., Baltimore [1].
Dunglison, Prof. Robley, Philadelphia [8].
Dutton, Thomas R., Hartford, Connecticut [1].
Dwight, James M. B., Esq., New Haven, Connecticut [5].
Dwight, Prof. T. W., Hamilton College, Clinton, New York [5].
Dwight, Timothy, Esq., New Haven, Connecticut [4].

E.

Eastabrook, Rev. Joseph, E. Tennessee College, Knoxville [1].
Easter, John D., Esq., Baltimore [6].
Ela, Joseph, Esq., Concord, New Hampshire [6].
Eliot, C. W., Cambridge, Massachusetts [8].
Elliot, Hon. William, Beaufort, South Carolina [1].

Elwyn, Dr. Alfred L., Philadelphia [1].
Ely, Charles Arthur, Esq., Elyria, Ohio [4].
Emerson, Prof. Alfred, Hudson, Ohio [7].
Emerson, George B., Esq., Boston [1].
EMMONS, Prof. EBENEZER, Albany, New York [1].
Emory, Maj. William H., U. S. A., Washington, D. C. [5].
Engelmann, Dr. George, St. Louis, Missouri [1].
Engstrom, A. B., Esq., Burlington, New Jersey [1].
Erni, Dr. Henri, New Haven, Connecticut [4].
Erwin, J. S., Marian, M'Dowell Co., North Carolina [3].
Espy, Prof. James P., Washington, D. C. [1].
Este, D. K., Esq., New Haven, Connecticut [4].
Eustis, Prof. H. L., Cambridge, Massachusetts [2].
Evans, Hon. Alexander, Elkton, Maryland [4].
Evans, Prof. John, M. D., Chicago, Illinois [5].
Evans, John, Radnor, Delaware Co., Pennsylvania [7].
Everett, Hon. Edward, Boston [2].
Ewbank, Thomas, Washington, D. C. [8].
Ewendberg, L. C., New Wied, Comal Co., Texas [7].
Ewing, Hon. Thomas, Lancaster, Ohio [5].

F.

Fairchild, Prof. J. H., Oberlin, Ohio [5].
Fairie, James, Prairie Mer Rouge, Louisiana [7].
Farnam, Prof. J. E., Georgetown, Kentucky [7].
Farrington, Alexander C., Esq., Newark, New Jersey [4].
Fearing, Dr. E. P., Nantucket, Massachusetts [8].
Felton, Prof. C. C., Cambridge, Massachusetts [2].
Felton, Frank Eliot, Esq., New York [4].
Felton, John B., Esq., San Francisco, California [2].
Felton, Samuel M., Philadelphia [2].
Ferris, Rev. Dr. Isaac N., New York [6].
Fillmore, Millard, Buffalo, New York [7].
Finley, Pres. W. P., Charleston [3].
Fisher, Thomas, Philadelphia, Pennsylvania [8].
Fitch, Alexander, Esq., Carlisle, New York [1].
Fitch, Rev. Enoch, Lyme, Connecticut [6].
Fitch, O. H., Ashtabula, Ohio [7].

Flagg, Lieut. H. C., U. S. N., New Haven, Connecticut [1].
Foote, John P., Esq., Cincinnati, Ohio [4].
Foote, Dr. Thomas M., Albany, New York [6].
Force, Col. Peter, Washington, D. C. [4].
Forrest, Rev. John, Charleston [3].
Forshey, Caleb Goldsmith, New Orleans [7].
Fosgate, Blanchard, M. D., Auburn, New York [7].
Foster, Prof. John, Schenectady, New York [1].
Foster, J. W., Esq., Brimfield, Massachusetts [1].
Fowle, William B., Esq., Boston [1].
Fowler, Prof. W. C. [5].
Fox, Rev. Charles, Grosse Ile, Michigan [7].
Frazer, Prof. John F., Philadelphia [1].
Fremont, Col. J. C., California [1].
Friedländer, Dr. Julius, Berlin, Prussia [7].
Frost, Rev. Adolph, Burlington, New Jersey [7].

G.

Gaillard, Dr. P. C., Charleston [2].
Gale, L. D., Washington, D. C. [8].
Gardin, Benjamin, Charleston [3].
Garland, Prof. L. C., Tuscaloosa, Alabama [1].
Garrigue, Rudolph, New York [7].
Gavit, John E., Esq., Albany, New York [1].
*Gay, Dr. Martin, Boston [1].
Gebhard, John, Esq., Schoharie, New York [1].
Geddings, Prof. E., Charleston [3].
Germain, Prof. Lewis J., Burlington, New Jersey [1].
Gerolt, Baron A. Von, Washington, D. C. [1].
Gibbes, Prof. L. R., Charleston [1].
Gibbes, Dr. Robert W., Columbia, South Carolina [1].
Gibbon, Dr. J. H., Charlotte, North Carolina [3].
Gibbs, Josiah W., New Haven, Connecticut [4].
Gibbs, Dr. Wolcott, New York [1].
Gilham, Prof. William, Lexington, Virginia [6].
Gillett, J., Esq., Cleveland, Ohio [6].
Gilliss, Lieut. J. M., U. S. N., Washington, D. C. [1].
*Gilmor, Robert, Esq., Baltimore [1].

Girard, Charles, Esq., Washington, D. C. [2].
Glynn, Com. James, U. S. N., New Haven, Connecticut [1].
Goadby, Dr. Henry, New York [6].
Gold, Theodore S., West Cornwall, Connecticut [4].
Goldmark, Dr., Vienna, Austria [4].
Goodsell, Dr. Thomas, Utica, New York [6].
Gould, Dr. A. A., Boston [1].
Gould, B. A., Esq., Boston [2].
Gould, Dr. B. A., Jr., Cambridge, Massachusetts [2].
Gourdin, Henry, Esq., Charleston [3].
Gourdin, Robert N., Esq., Charleston [3].
Graham, George, Esq., Cincinnati, Ohio [5].
Graham, Col. James D., U. S. A., Washington, D. C. [1].
Gray, Prof. Asa, Cambridge, Massachusetts [1].
Gray, H., Esq., Springfield, Massachusetts [6].
Gray, Dr. James H., Springfield, Massachusetts [6].
Green, Dr. B. F., Chestertown, Maryland [2].
Green, James, Esq., New York [1].
Green, Dr. Traill, Easton, Pennsylvania [1].
Greene, Dr. Benjamin D., Boston [1].
Gregg, William, Esq., Charleston [3].
*Griffith, Dr. Robert E., Philadelphia [1].
Grinnan, A. G., Madison Court-House, Virginia [7].
Gröneweg, Lewis, Germantown, Ohio [7].
Grosvenor, H. C., Esq., Cincinnati, Ohio [5].
Guest, William E., Esq., Ogdensburg, New York [6].
Guilford, Hon. Nathan, Cincinnati, Ohio [4].
Gummere, Samuel J., Burlington, New Jersey [7].
Guyot, Prof. A., Cambridge, Massachusetts [1].

H.

Hackley, Prof. Charles W., New York [4].
Hague, John M., Esq,, Newark, New Jersey [6].
Hague, William W., Esq., Newark, New Jersey [6].
Haldeman, Prof. S. S., Columbia, Pennsylvania [1].
*Hale, Dr. Enoch, Boston [1].
Hall, Edward, Esq., San Francisco, California [1].
Hall, Dr. Frederick, Washington, D. C. [1].

HALL, Prof. JAMES, Albany, New York [1].
Hall, Joel, Athens, Illinois [7].
Hall, N. K., Buffalo, New York [7].
Hall, Rev. William, Mobile, Alabama [3].
Halleck, Lieut. H. W., U. S. A., San Francisco, California [1].
Hallowell, Benjamin, Alexandria, Va. [7].
Hamel, Dr., St. Petersburg, Russia [8].
Hamilton, Dr., Mobile, Alabama [3].
Hammond, J. F., M. D., U. S. A., Fort Columbus, New York [7].
Hammond, Ogden, Esq., Charleston [3].
Hamnett, J., Meadville, Pennsylvania [7].
Hance, Ebenezer, Morrisville, Bucks Co., Pennsylvania [7].
Handy, J. P., Esq., Cleveland, Ohio [6].
Hardcastle, Edmund L. F., U. S. A., Washington, D. C. [7].
Hare, Dr. Robert, Philadelphia [1].
Harlan, Prof. Joseph G., Haverford, Pennsylvania [8].
*Harlan, Dr. Richard, Philadelphia [1].
Harris, C. Townsend, Esq., New York [4].
Harris, Charles T., Esq., New York [4].
Harris, Hon. Ira, Albany, New York [6].
Harris, J. O., Ottawa, La Salle Co., Illinois [7].
Harris, Dr. Thaddeus W., Cambridge, Massachusetts [1].
Harris, Prof. W. L., Delaware, Ohio [7].
Hart, Simeon, Esq., Farmington, Connecticut [1].
Harte, R. E., Columbus, Ohio [7].
Hartley, M. B., Esq., Montreal, Canada [1].
Hartwell, Charles, Esq., Lincoln, Massachusetts [6].
Harvey, Hon. Matthew, Hopkinton, New Hampshire [1].
Harvey, Prof. William H., Trinity College, Dublin [3].
Haskins, R. W., Esq., Buffalo, New York [6].
Haupt, H., Esq., Harrisburg, Pennsylvania [3].
Hawkesworth, Prof. W., Charleston [3].
Hay, Dr. George, Barnwell, South Carolina [3].
HAYDEN, C. B., Esq., Smithfield, Virginia [1].
*Hayden, Dr. H. H., Baltimore [1].
Hayes, John L., Esq., Washington, D. C. [3].
Haynesworth, Dr. ——? Sumpterville, South Carolina [1].
Hays, Dr. Isaac, Philadelphia [1].

Hayward, James, Esq., Boston [1].
Hedrick, Prof. B. S., Chapel Hill, North Carolina [8].
Henry, Prof. Joseph, Washington, D. C. [1].
Herbert, Alfred, Washington, D. C. [8].
Herrick, Edward C., Esq., New Haven, Connecticut [1].
Hiester, John P., M. D., Reading, Pennsylvania [7].
Hildreth, Dr. S. P., Marietta, Ohio [1].
Hilgard, Julius E., Esq., U. S. Coast Survey, Washington [4].
Hilgard, Dr. T. C., Philadelphia, Pennsylvania [8].
Hill, B. L., Berlinsville, Ohio [7].
Hill, Nicholas, Jr., Albany, New York [6].
Hill, S. W., Esq., Eagle Harbor, Lake Superior [6].
Hill, Rev. Thomas, Waltham, Massachusetts [3].
Hillard, George S., Esq., Boston [3].
Hillhouse, William, Esq., New Haven, Connecticut [4].
Hincks, Rev. William, London, England [1].
Hirzel, Henri, Lausanne, Switzerland [4].
Hiscox, Gardner D., New York [4].
HITCHCOCK, Pres. EDWARD, Amherst, Massachusetts [1].
Hitchcock, Edward, Jr., Amherst, Massachusetts [4].
Holbrook, Dr. John E., Charleston [1].
Holmes, F. S., Esq., Charleston [3].
Holton, Dr. David P., New York [6].
Holyoke, F. E., Esq., Salem, Massachusetts [4].
Holyoke, Francis, Esq., Salem, Massachusetts [6].
Hopkins, J. A., Milwaukee, Wisconsin [8].
Hopkins, William, Esq., Auburn, New York [5].
Hopkins, Prof. W. F., Annapolis, Maryland [7].
Hord, Kellis [7].
Horlbeck, Dr. E., Charleston [3].
Horsford, Prof. E. N., Cambridge, Massachusetts [1].
*HORTON, Dr. WILLIAM, Craigville, Orange Co., New York [1].
Hotchkiss, Jedediah, Bridgewater, Virginia [6].
*HOUGHTON, Dr. DOUGLAS, Detroit, Michigan [1].
Howell, Robert, Esq., Nichols, Tioga Co., New York [6].
Hoy, Philo R., M. D., Racine, Wisconsin [7].
Hoyt, J. W., Cincinnati, Ohio [8].
Hubbard, Prof. J. S., Washington, D. C. [1].

Hubbard, Prof. Oliver P., Hanover, New Hampshire [1].
Hume, Dr. W., Charleston [2].
Humphrey, S. Dwight, Esq., New York [6].
Humphrey, Wm. F., New Haven, Connecticut [7].
Hun, Dr. Thomas, Albany, New York [4].
Hunt, Lieut. E. B., U. S. A., Washington, D. C. [2].
Hunt, Thomas S., Esq., Montreal, Canada [1].
Hunt, Hon. Washington, Albany, New York [6].
Hunton, Dr. Ariel, Hyde Park, Maryland [4].

I.

Ingalls, Dr. T. R., Greenwich, Washington Co., New York [1].
Ives, Dr. Eli, New Haven, Connecticut [4].
Ives, Dr. Nathan B., New Haven, Connecticut [4].

J.

JACKSON, Dr. CHARLES T., Boston [1].
Jackson, Dr. R. M. S., Alexandria, Pennsylvania [1].
Jackson, Dr. Samuel, Philadelphia [1].
Jacobs, Prof. M., Gettysburg, Pennsylvania [8].
James, U. P., Esq., Cincinnati, Ohio [5].
Jay, Dr. John C., New York [1].
Jayne, J. W., Esq., Cincinnati, Ohio [5].
Jeffrey, R. W., M. D., U. S. N., Pensacola, Florida [7].
Jeffers, W. V., U. S. N., Bridgeton, New Jersey [4].
Jenkins, John F., Esq., N. Salem, Washington Co., New York [6].
Jenkins, Thornton A., U. S. N., Washington, D. C. [7].
Jenks, W. P., Esq., Middleborough, Massachusetts [2].
Jenner, Solomon, Esq., New York [4].
Jennings, N. R., Esq., New Orleans [4].
Jervey, Dr. J. P., Charleston [3].
Jewett, Prof. C. C., Washington, D. C. [2].
Johnson, Alexander S., Esq., New York [6].
Johnson, Hosmer A., M. D., Chicago, Illinois [7].
Johnson, James, Esq., Charleston [3].
Johnson, Dr. Joseph, Charleston [3].
Johnson, S. W., Esq., Deer River, Lewis Co., New York [4].
Johnson, Samuel, New Haven, Connecticut [7].

Johnson, Rev. Wm., Tuscaloosa, Alabama [7].
*JOHNSON, Prof. W. R., Washington, D. C. [1].
Johnson, William C., Esq., Utica, New York [6].
Johnston, Prof. John, Middletown, Connecticut [1].
Johnston, Stephen, M. D., Platte City, Missouri [7].
Jones, Edward, Esq., Charleston [3].
Jones, Lieut. Catesby Ap. R., U. S. N. [8].
Jones, Dr. William L., Athens, Georgia [3].
Joy, Prof. C. A., Ovid, New York [8].
Judd, Orange, Esq., New Haven, Connecticut [4].

K.

Keeley, Prof. G. W., Waterville, Maine [1].
Keith, Prof. Reuel, Washington, D. C. [5].
Kellogg, Orson, Esq., New York [1].
Kelly, Edwin, M. D., Elyria, Ohio [7].
Kempton, John, Esq., New York [4].
Kendall, David, Rochester, New York [8].
Kendall, E. Dwight, Esq., Cambridge, Massachusetts [4].
Kendall, E. Otis, Esq., Philadelphia [1].
Kendrick, Maj. H. L., West Point, New York [1].
Kennedy, Alfred L., M. D., Philadelphia, Pennsylvania [7].
Kent, Edward N., New York [8].
Kerr, George H., Esq., Franklin, Delaware Co., New York [6].
Kimball, Prof. D. A., Warren, Massachusetts [6].
King, Charles, LL.D., Columbia College, New York [6].
King, Dr. Henry, St. Louis, Missouri [1].
King, Hon. Mitchell, Charleston [3].
Kingsley, Prof. C., Meadville, Pennsylvania [6].
Kirkpatrick, James A., Philadelphia, Pennsylvania [7].
Kirkpatrick, J., Ohio City, Ohio [7].
Kirkwood, Daniel, Newark, Delaware [7].
Kirtland, Dr. J. P., Cleveland, Ohio [1].
Kitchell, Dr. William, Newark, New Jersey [6].
Kite, Thomas, Cincinnati, Ohio [5].
Klipstein, Dr. Louis F., St. James, Santee, S. Carolina [4].
Kneeland, Dr. Samuel, Jr., Boston [1].
Kurtz, Lieut. J. D., U. S. A., Charleston [3].

L.

Lamoreux, Dr. M. W., Schenectady, New York [6].
Lane, ——? Esq., Milledgeville, Georgia [3].
Lane, E., Sandusky, Ohio [3].
Langdon, W. Chauncey, Esq., Washington, D. C. [5].
Lansing, Hon. Gerritt Y., Albany, New York [6].
Lapham, Increase A., Esq., Milwaukee, Wisconsin [3].
*Lasel, Prof. Edward, Williamstown, Massachusetts [1].
Latham, Prof. Richard P., Richmond, Virginia [8].
Lathrop, Stephen P., M. D., Beloit, Wisconsin [7].
Lawrence, George N., New York [7].
Lawrence, Dr. William, Boston [2].
Lea, Isaac, Esq., Philadelphia [1].
Leavenworth, Dr. M. C., Waterbury, Connecticut [1].
Le Conte, Maj. John, Philadelphia [1].
Le Conte, Dr. John L., Philadelphia [1].
Leconte, Prof. John, Athens, Georgia [3].
Leconte, Dr. Joseph, Macon, Georgia [3].
*Lederer, Baron von, Washington, D. C. [1].
Lee, John C., Esq., Salem, Massachusetts [1].
Lee, Capt. T. J., U. S. A., Washington, D. C. [5].
Leeds, S. P., Esq., Brooklyn, New York [6].
Leffingwell, Prof. E. H., Columbia, Missouri [5].
Lefroy, Capt. J. H., R. A., Toronto, Canada West [6].
Leidy, Joseph, Philadelphia, Pennsylvania [7].
Leonard, F. B., Esq., Lansingburgh, New York [6].
Leonard, Dr. Frederick B., Washington, New York [1].
Leonard, Rev. Levi, Dublin, New Hampshire [6].
Lesesne, Henry D., Esq., Charleston [3].
Lesley, J. P., Esq., Milton, Massachusetts [2].
Lesley, Joseph, Jr., Philadelphia, Pennsylvania [8].
Lesley, Dr. William W., Monticillo, Missouri [8].
Lieber, Dr. Francis, Columbia, South Carolina [2].
Lieber, Oscar M., Columbia, South Carolina [8].
Linck, Dr. Christian, Philadelphia [2].
*Lindsley, Rev. James H., Stafford, Connecticut [1].
Linklaen, Ledyard, Esq., Cazenovia, New York [1].
Linsley, Dr. J. B., Nashville, Tennessee [1].

Lintner, J. Albert, Esq., Schoharie, New York [6].
Lippitt, Prof. E. S., Cincinnati, Ohio [5].
Lischka, Emile, Esq., Berlin, Prussia [1].
Livermore, Rev. A. A., Cincinnati, Ohio [5].
Locke, Dr. J. T., Nashua, New Hampshire [2].
Locke, Luther F., M. D., Nashua, New Hampshire [7].
Logan, William E., Esq., Montreal, Canada [1].
Loomis, Prof. Elias, New York [1].
Loomis, Silas L., North Bridgewater, Massachusetts [7].
Lord, Asa D., M. D., Columbus, Ohio [7].
Lovering, Prof. Joseph, Cambridge, Massachusetts [2].
Lyell, Sir Charles, London, England [1].
Lyman, Rev. Addison, Geneseo, Illinois [6].
Lyman, Chester S., Esq., New Haven, Connecticut [4].
Lyman, J., Esq., Lenox, Massachusetts [6].
Lynch, Rev. Dr. P. N., Charleston [2].
Lyon, Hon. Caleb, Lyonsdale, New York [8].

M.

McAlpine, William J., Esq., Albany, New York [6].
McCall, Col. George A., Philadelphia, Pennsylvania [7].
M'Conihe, Hon. Isaac, Troy, New York [4].
McCulloch, Prof. R. S., New York [1].
McDonald, Marshall, New Creek Depot, Hampshire Co., Virginia [7].
McElroy, Rev. Jas., Delaware, Ohio [7].
McEuen, Dr. Thomas, Philadelphia [1].
McFarlan, Henry, Dover, New Jersey [8].
McKay, Prof. C. F., Athens, Georgia [3].
Maclean, Prof. George M., New Albany, Indiana [5].
McMinn, J. M., Williamsport, Lycoming Co., Pennsylvania [7].
M'Naughton, Dr. James, Albany, New York [4].
Macrae, Lieut. Archibald, North Carolina [8].
McRae, John, Esq., Charleston [3].
MacWhorter, Alexander, Esq., New Haven, Connecticut [4].
Maffit, Lieut. J. N., U. S. N. [3].
Major, James, U. S. N., Washington, D. C. [7].
Mandeville, Rev. H., Albany, New York [6].

Manigault, Edward, Esq., Charleston [3].
Mansfield, E. D., Cincinnati, Ohio [4].
Mantell, Reginald Neville, Esq., London [2].
March, Dr. Alden, Albany, New York [4].
Markham, Jesse, Salem, Ohio [7].
Markoe, Francis, Jr., Esq., Washington, D. C. [1].
*Marsh, Dexter, Esq., Greenfield, Massachusetts [1].
Marshall, Prof. Charles, Indiana [4].
Martin, Rev. Benjamin N., Albany, New York [6].
Mason, Prof. Cyrus, New York [4].
Mason, Rev. Francis, Maulmain, India [7].
Mason, Isaac N., Cleveland, Ohio [7].
Mather, William W., Esq., Columbus, Ohio [1].
Mathiot, George, Washington, D. C. [8].
Matthews, Prof. T. J., Oxford, Ohio [5].
Mattison, Hiram, New York [7].
Mauran, Dr. J., Providence, Rhode Island [2].
Maury, Lieut. M. F., Washington, D. C. [1].
Maverick, Augustus, Esq., New York [4].
Maynard, Alleyne, M. D., Cleveland, Ohio [7].
Mead, Orlando, Esq., Albany, New York [6].
Means, Prof. A., Oxford, Georgia [5].
Medbury, E., Esq., Columbus, Ohio [5].
Meech, C. W., Washington, D. C. [8].
Meek, T. B., Esq., Owenboro', Kentucky [6].
Melsheimer, Dr. F. E., Dover, York Co., Pennsylvania [1].
Merrick, Prof. F., Delaware, Ohio [1].
Merrick, S. V., Esq., Philadelphia [1].
Michel, Dr. M., Charleston [2].
Michelotti, M. J., Turin, Piedmont, Italy [1].
Miles, Dr. F. T., Charleston [4].
Miller, H. B., Esq., New York [1].
Miller, Prof. W. A., Albany, New York [6].
Millington, Prof. John, Oxford, Mississippi [1].
Mills, B. F., Baraboo, Sauk Co., Wisconsin [7].
Mitchel, Prof. E., Chapel Hill, North Carolina [1].
Mitchel, Prof. O. M., Cincinnati, Ohio [3].
Mitchell, Prof. J. B., East Tennessee University [7].

Mitchell, Dr. J. K., Philadelphia [1].
Mitchell, Maria, Nantucket, Massachusetts [4].
Mitchell, Hon. William, Nantucket, Massachusetts [2].
Molinard, Prof. J., Albany, New York [6].
Monroe, Rev. Nathan, Bradford, Massachusetts [6].
Monson, Dr. Alfred S., New Haven, Connecticut [4].
Moody, L. A., Esq., Chicopee, Massachusetts [5].
Moore, George H., New York [8].
Moore, Rev. Thomas V., Richmond, Virginia [7].
Mordecai, Alfred, U. S. A., Washington, D. C. [7].
Morfit, Campbell, Baltimore, Maryland [7].
Morris, Rev. John G., Baltimore [1].
Morris, Margaretta H., Germantown, Pennsylvania [4].
Morris, O. W., Esq., New York [1].
Morris, Rev. R., Jackson, Mississippi [2].
*Morton, Dr. S. G., Philadelphia [1].
Moss, Theodore F., Esq., Philadelphia [2].
Moultrie, Dr. J., Charleston [1].
Moultrie, Dr. William L., Charleston [2].
Munsell, J., Esq., Albany, New York [6].
Murphy, J. W., Esq., Troy, New York [6].
Mutter, Dr. Thomas D., Philadelphia [1].

N.

Nash, Rev. J. A., Amherst, Massachusetts [6].
Napoli, Prof. Raphael, Naples [8].
Nelson, J. P., New Windsor, Carroll Co., Maryland [7].
Newberry, Dr. John S., Cleveland, Ohio [5].
Newland, John, Albany, New York [7].
Newman, Dr. ——? Huntsville, Alabama [3].
Newton, Rev. E. H., Cambridge, New York [1].
Newton, Hubert A., Esq., Sherburne, New York [6].
Newton, John, Knox Hill, Walton Co., Florida [7].
Nichols, Dr. Andrew, Danvers, Massachusetts [1].
Nichols, Dr. James R., Haverhill, Massachusetts [7].
*Nicollet, J. N., Esq., Washington, D. C. [1].
Northrop, Richard H., Esq., Albany, New York [6].
Norton, Rev. Niram, Meadville, Pennsylvania [8].

*Norton, Prof. J. P., New Haven, Connecticut [1].
Norton, Prof. W. A., New Haven, Connecticut [6].
Norwood, Dr. J. G., New Harmony, Indiana [1].
Nott, Dr. J. C., Mobile, Alabama [3].

O.

*Oakes, William, Esq., Ipswich, Massachusetts [1].
Oeland, John C., Fort Prince, Spartanburg District, S. C. [7].
Ogier, Dr. T. L., Charleston [3].
Olcott, Thomas W., Esq., Albany, New York [2].
Oliver, J. D., Esq., Lynn, Massachusetts [2].
Oliver, James Edward, Cambridge, Massachusetts [7].
Olmsted, Alexander F., New Haven, Connecticut [4].
Olmsted, Charles H., Esq., E. Hartford, Connecticut [1].
Olmsted, Prof. D., New Haven, Connecticut [1].
*Olmsted, Denison, Jr., Esq., New Haven, Connecticut [1].
Opdyke, George, New York [8].
Orvis, Joseph U., Troy, New York [6].
Osborn, A., Esq., Albany, New York [1].
Osborne, E., Esq., Sandusky, Ohio [5].
Osgood, Rev. Samuel, New York [8].
Owen, Dr. David D., New Harmony, Indiana [1].
Owen, Prof. Richard, New Harmony, Indiana [5].
Owen, Hon. Robert D., New Harmony, Indiana [1].

P.

Page, Dr. Charles G., Washington, D. C. [1].
Paine, H. B., Esq., Cleveland, Ohio [6].
Paine, Dr. H. D., Albany, New York [6].
Painter, Minshall, Lima, Delaware Co., Pennsylvania [7].
Palmer, Charles H., Rome, Michigan [6].
Park, Rev. Roswell C., Norwich, Connecticut [1].
Parker, Hon. Amasa J., Albany, New York [6].
Parker, Charles, Esq., Charleston [3].
*Parkman, Dr. Samuel, Boston [1].
Parry, Dr. Charles C., Davenport, Iowa [6].
Parsons, Theophilus, Esq., Cambridge, Massachusetts [1].
Peabody, Francis, Esq., Salem, Massachusetts [1].

Peale, Titian R., Esq., Washington, D. C. [1].
Peet, Dr. H. P., New York [6].
Peirce, Prof. Benjamin, Cambridge, Massachusetts [1].
Peirce, David S., Esq., Albany, New York [6].
Pendergast, John G., Smithville, Jefferson Co., New York [1].
Pendleton, A. G., U. S. N., Washington, D. C. [7].
Pendleton, Dr. —— ? Sparta, Georgia [3].
Perkins, Prof. G. R., Albany, New York [1].
Perkins, Dr. H. C., Newburyport, Massachusetts [1].
Perkins, Rev. Justin, Oroomiah, Persia [1].
Perry, A. F., Columbus, Ohio [7].
Perry, Horace, Esq., Cleveland, Ohio [6].
Phelps, Dr. Edward E., Windsor, Vermont [4].
Phelps, S. L., U. S. N., Washington, D. C. [8].
Phillips, William, Augusta, Georgia [7].
Pickering, Dr. Charles, Boston [1].
Pitcher, Dr. Zina, Detroit, Michigan [1].
Plant, I. C., Esq., Macon, Georgia [3].
Pohlman, Rev. Dr. Henry, Albany, New York [6].
Porcher, Prof. F. A., Charleston [3].
Porcher, Dr. Francis Peyre, Charleston [3].
Porter, B. F., Esq., Charleston [3].
Porter, Prof. John A., New Haven, Connecticut [4].
Porter, Prof. Noah, New Haven, Connecticut [4].
Porter, Prof. Thomas C., Lancaster, Pennsylvania [5].
Potter, Rev. Dr. H., Albany, New York [6].
Poulson, Charles A., Esq., Philadelphia [1].
Pourtales, L. F., Esq., Washington, D. C. [1].
Powers, A. E., Esq., Lansingburg, New York [1].
Prentice, E. P., Esq., Albany, New York [2].
Prescott, Dr. William, Concord, New Hampshire [1].
Prioleau, Dr. Thomas, Charleston [3].
Probasco, Henry, Esq., Cincinnati, Ohio [5].
Prout, H. A., M. D., St. Louis, Missouri [7].
Pruyn, J. V. L., Esq., Albany, New York [1].
Pursell, Rt. Rev. Bishop, Cincinnati, Ohio [4].
Pybas, Benjamin, Tuscumbia, Alabama [7].
Pynchon, T. R., Esq., Stockbridge, Massachusetts [1].

R.

Rainey, Thomas, Esq., New York [4].
Randall, S. S., Esq., Albany, New York [2].
Randell, John, New York [8].
Ravenel, Dr. Edmond, Charleston [1].
Ravenel, H., Esq., Charleston [3].
Ravenel, Dr. Henry, Charleston [3].
Ravenel, Dr. St. Julien, Charleston [1].
Ray, Dr. Joseph, Cincinnati, Ohio [4].
Raymond, Dr. Charles H., Cincinnati, Ohio [5].
Read, M. C., Hudson, Ohio [7].
Redfield, John H., Esq., New York [1].
Redfield, William C., Esq., New York [1].
Reed, Dr. Stephen, Pittsfield, Massachusetts [1].
Rehfus, Lewis, Esq., Cincinnati, Ohio [5].
Renwick, Prof. James, New York [1].
Rhees, William J., Washington, D. C. [8].
Rice, De Witt C., Albany, New York [7].
Rice, Henry, North Attleborough, Massachusetts [7].
Rice, Rev. N. L., Cincinnati, Ohio [4].
Richards, Z., Washington, D. C. [7].
Richmond, A. D., Meadville, Pennsylvania [8].
Riddell, Dr. John L., New Orleans [1].
Riddell, Wm. P., New Orleans, Louisiana [7].
Riell, Lieut., U. S. N. [3].
Ripley, Hezekiah W., Esq., New York [6].
Rhett, James, Esq., Charleston [3].
Robb, Prof. James, M. D., Fredericton, New Brunswick [4].
Roberts, Algernon S., Esq., Philadelphia [1].
Roberts, Solomon W., Esq., Philadelphia [2].
Roberts, W. Milnor, Pittsburg, Pennsylvania [5].
Robertson, Dr. F. M., Charleston [3].
Robinson, Prof. H. N., Cincinnati, Ohio [5].
Robinson, J. R., Esq., Mansfield, Ohio [4].
Roemer, Dr. F., Berlin, Prussia [1].
ROGERS, Prof. HENRY D., Boston [1].
*Rogers, Prof. James B., Philadelphia [1].
ROGERS, Prof. ROBERT E., Charlottesville, Virginia [1].

Rogers, Prof. S. T., Chestertown, Maryland.
Rogers, Prof. W. B., Boston [1].
Rood, Ogden N., New Haven, Connecticut [7].
Root, Prof. O., Hamilton College, Clinton, New York [1].
Ross, A. C., Esq., Zanesville, Ohio [6].
Rousseau, Henry, Esq., Albany, New York [6].
Ruggles, Lieut. ——? Detroit, Michigan [1].
Ruggles, Samuel B., Esq., New York [6].
Ruggles, Prof. William, Washington, D. C. [8].
Runkle, J. D., Esq., Cambridge, Massachusetts [2].
Ruschenberger, Dr. W. S. W., Philadelphia [1].
Russell, Rev. John L., Hingham, Massachusetts [1].

S.

Saeman, Louis, Berlin, Prussia [2].
Safford, Prof. J. M., Lebanon, Tennessee [6].
Sager, Prof. Abraham, Ann Arbor, Michigan [6].
Salisbury, Prof. Edward E., New Haven, Connecticut [4].
Salisbury, Prof. J. H., Albany, New York [2].
Sanders, Prof. J. Milton, Memphis, Tennessee [5].
Sanford, R. R., Riga, New York [7].
Sanford, S. N., Esq., Granville, Ohio [6].
Saxton, Joseph D., Esq., Washington, D. C. [1].
Saynish, Dr. Lewis, Blossburg, Pennsylvania [1].
Scarborough, Rev. George, Owensburg, Kentucky [2].
Schaeffer, Prof. George C., Washington, D. C. [1].
Schank, Dr. J. Stilwell, Princeton, New Jersey [4].
Schoolcraft, Henry R., Washington, D. C. [7].
Schott, Arthur C. V., Washington, D. C. [8].
Schott, Charles A., Washington, D. C. [8].
Scott, J. W., Oxford, Ohio [7].
Scoville, John, Esq., Salisbury, Massachusetts [6].
Scriven, Dr. T. P., Savannah, Georgia [3].
Seely, Charles A., Esq., Rochester, New York [6].
Selden, George M., Esq., Troy, New York [6].
Selden, Silas R., Esq., New Haven, Connecticut [4].
Sellers, Geo. Escol, Cincinnati, Ohio [7].
Sessions, John, Esq., Albany, New York [6].

Sestini, Prof. Benedict, Georgetown, D. C. [8].
Seward, Hon. William H., Auburn, New York [1].
Shaefer, P. W., Pottsville, Pennsylvania [4].
Shaffer, David H., Cincinnati, Ohio [7].
Shaler, Dr. N. B., Newport, Kentucky [5].
Shane, J. D., Lexington, Kentucky [7].
Shaw, Rev. James, Newburg, Ohio [7].
Shepard, Prof. C. U., New Haven, Connecticut [4].
Shepard, Rev. G. C., Boston [3].
Shepherd, Rev. J. Avery, Scuppernong, North Carolina [7].
Sherman, Prof. S. S., Howard College, Alabama [3].
Shippen, William, Washington, D. C. [8].
Shumard, B. F., M. D., St. Louis, Missouri [7].
Shurtleff, Dr. N. B., Boston [1].
Sill, Hon. Elisha, Cuyahoga Falls, Ohio [6].
Silliman, Prof. Benjamin, New Haven, Connecticut [1].
Silliman, Prof. Benjamin, Jr., New Haven, Connecticut [1].
Silsby, Horace, Blue Hill, Maine [7].
Simmons, Daniel, Esq., Paris Hill, New York [6].
Sismondi, Dr. Eugene, Turin, Piedmont [1].
Skilton, Dr. Avery J., Troy, New York [6].
Slack, Dr. Elijah, Cincinnati, Ohio [5].
Smallwood, Charles, M. D., St. Martin, Isle Jesus, Canada East [7].
Smead, Morgan J., P. D., Williamsburg, Virginia [7].
Smith, Prof. A. W., Middletown, Connecticut [4].
Smith, Erastus, Esq., Hartford, Connecticut [1].
Smith, Capt. E. R., U. S. A. [8].
Smith, George, Haverford, Delaware Co., Pennsylvania [7].
Smith, George W. L., Esq., Troy, New York [6].
Smith, Prof. Hamilton L., Cleveland, Ohio [5].
Smith, J. Bryant, M. D., New York [7].
Smith, Prof. J. Lawrence, Louisville [1].
*Smith, J. V., Esq., Cincinnati, Ohio [5].
Smith, Dr. J. V. C., Boston [1].
Smith, John Chappall, New Harmony, Indiana [7].
Smith, Oliver, Esq., New York [1].
Smith, Rev. Thomas, D. D., Charleston [7].
Snell, Prof. Eben S., Amherst, Massachusetts [2].

Snow, Charles B., Washington, D.C. [8].
Sommers, Rev. Dr., Charleston [3].
Sowell, Dr. J. F., Athens, Georgia [3].
Sparks, Jared, Cambridge, Massachusetts [2].
Spear, Charles V., Pittsfield, Massachusetts [4].
Spencer, C. A., Esq., Canastota, New York [6].
Spencer, Thomas, Philadelphia [8].
Spooner, Edward A., Plymouth, Massachusetts [4].
Sprague, Charles Hill, Malden, Massachusetts [7].
Squier, E. G., Esq., New York [1].
St. John, Prof. Samuel, Cleveland, Ohio [1].
Stadmuller, Ludwig, Bristol, Connecticut [4].
Stannard, Benjamin, Esq., Cleveland, Ohio [6].
Stansbury, Capt. Howard, U. S. A., Washington, D. C. [6].
Stearns, Prof. E. J., Annapolis, Maryland.
Stebbins, Dr. Richard, Springfield, Massachusetts [4].
Stebbins, Rev. Rufus P., Meadville, Pennsylvania [2]
Steiner, Dr. Lewis H., Baltimore, Maryland [7].
Stephens, A. H., Esq., New York [1].
Stetson, Charles, Cincinnati, Ohio [4].
Steuart, Prof. A. P., Lebanon, Tennessee [6].
Stevens, Maj. J. J., U. S. A., Washington, D. C. [6].
Stevens, Robert P., M. D., Ceres, Alleghany Co., New York [7].
Stewart, Prof. Wm. M., Clarksville, Tennessee [7].
Stillman, Dr. C. H., Plainfield, New Jersey [8].
Stillman, Dr. J. D. B., New York [8].
Stillman, Thomas B., New York [8].
Stoddard, Prof. J. W., Oxford, Ohio [5].
Stodder, Charles, Esq., Boston [1].
Stone, Charles S., Esq., Trenton, New Jersey [4].
Stone, L., Esq., Cleveland, Ohio [4].
Storer, Dr. D. H., Boston [1].
Stuntz, Geo. R., Lancaster, Wisconsin [7].
Sullivant, Joseph, Columbus, Ohio [7].
Sullivant, Wm. S., Columbus, Ohio [7].
Sumner, Hon. Charles, Boston [2].
Sumner, George, Boston [8].
Sutherland, Prof., Montreal, Canada [6].

Swan, Lansing B., Rochester, New York [8].
Swift, Dr. William, U. S. N., New York [1].
Swinburne, John, Esq., Albany, New York [6].

T.

Tabor, Azor, Esq., Albany, New York [6].
Talcott, Andrew, Richmond, Virginia [7].
*Tallmadge, Hon. James, New York [1].
Taylor, James, Esq., Albany, New York [6].
Taylor, Dr. Julius S., Carrolton, Montgomery Co., Ohio [1].
Taylor, Morse K., M. D., Brooklyn, Jackson Co., Michigan [7].
Taylor, M. R., Brooklyn, Michigan [7].
*TAYLOR, RICHARD C., Esq., Philadelphia [1].
Taylor, W. J., Esq., Philadelphia [6].
Tenney, Benjamin J., Kingston, New York [6].
Terlecki, Ignatius, Paris [8].
*Teschemacher, J. E., Esq., Boston [1].
Tewis, Robert C., Esq., Shelbyville, Kentucky.
Thomas, W. H. B., Cincinnati, Ohio [7].
Thomas, William S., Norwich, New York [6].
Thompson, Dr. Alexander, Aurora, New York [6].
Thompson, Aaron R., New York [1].
Thompson, John Edgar, Esq., Philadelphia [1].
Thompson, Robert, Columbus, Ohio [7].
Thompson, Hon. Waddy, Greenville, South Carolina [3].
Thompson, Rev. Z., Burlington, Vermont [1].
Thurber, George, Esq., Providence, Rhode Island [1].
Thurston, E. M., Charleston, Maine [7].
Tiffany, Charles C., Esq., Baltimore [6].
Tiffany, Prof. O. H., Carlisle, Pennsylvania [5].
Tingley, Prof. Joseph, Greencastle, Indiana [5].
Tolman, Albert, Esq., Pittsfield, Massachusetts [6].
Torrey, Dr. John, New York [1].
Torrey, Prof. Joseph, Burlington, Vermont [2].
Totten, Gen. J. G., U. S. A., Washington, D. C. [1].
Town, Salem, New Albany, Indiana [7].
Townsend, Dr. —— ? John's Island, South Carolina [3].
Townsend, Hon. Franklin, Albany, New York [4].

*Townsend, John K., Esq., Philadelphia [1].
Treadwell, Daniel, Cambridge, Massachusetts [2].
Treadwell, Prof. J., Cambridge, Massachusetts [1].
TREGO, CHARLES B., Esq., Philadelphia [1].
Trenholm, George A., Esq., Charleston [3].
*Troost, Dr. Gerard, Nashville, Tennessee [1].
Troost, Lewis, Esq., Nashville, Tennessee [1].
Trumbull, James H., Esq., Stonington, Connecticut [4].
Tuomey, Prof. M., Tuscaloosa, Alabama.
Turner, Wm. C., Cleveland, Ohio [7].
Turner, Wm. W., Washington, D. C. [7].
Tuthill, Franklin, M. D., New York [8].
Twining, Prof. Alexander C., Erie, Pennsylvania [6].
*Tyler, Rev. Edward R., New Haven, Connecticut [1].
Tyler, Moses, Detroit, Michigan [7].

V.

Vail, Prof. Hugh, Haverford, Pennsylvania [8].
Vancleve, John W., Dayton, Ohio [1].
Van Lennep, Rev. H., Constantinople, Turkey [1].
Van Derzee, W. S., M. D., Buffalo, New York [7].
Van Pelt, Wm., M. D., Williamsville, Erie Co., New York [7].
Van Tuyle, Dr. Henry, Dayton, Ohio [6].
Van Vliet, Stewart, U. S. A., Brownsville, Texas [7].
*VANUXEM, LARDNER, Esq., Bristol, Pennsylvania [1].
Vaughan, Daniel, Esq., Newtown, Scott Co., Kentucky [5].
Vaux, William S., Esq., Philadelphia [1].
Venable, Prof. Charles, Virginia [4].

W.

Wadsworth, James S., Esq., Genesee, New York [2].
Wailes, Col. B. L. C., Washington, Mississippi [1].
Walker, Rev. Jas. B., Mansfield, Ohio [7].
Walker, Joseph (late U. S. A.), Platte City, Missouri [7].
*Walker, Sears C., Esq., Washington, D. C. [1].
Walker, Hon. Timothy, Cincinnati, Ohio [4].
Ward, James W., Esq., Cincinnati, Ohio [5].
Ward, Dr. Matthew A., Athens, Georgia [3].

Warder, Dr. J. A., Cincinnati, Ohio [4].
Waring, Charles B., Esq., New York [6].
Waring, Samuel C., Esq., New York [6].
Warren, Dr. J. Mason, Boston [2].
Warren, Dr. John C., Boston [1].
Webber, Dr. Samuel, Charlestown, New Hampshire [1].
*Webster, H. B., Esq., Albany, New York [1].
*Webster, Dr. J. W., Cambridge, Massachusetts [1].
*Webster, M. H., Esq., Albany, New York [1].
Webster, Nathan B., Portsmouth, Virginia [7].
Weddell, H. P., Esq., Cleveland, Ohio [6].
Weed, Monroe, Esq., Le Roy, New York [6].
Weld, Henry Thomas, Jr., Esq., Mount Savage Works, Maryland [1].
Weld, Mason C., New Haven, Connecticut [4].
Wells, David A., Springfield, Massachusetts [2].
Wells, Dr. Thomas, New Haven, Connecticut [4].
Wentworth, Prof. Erastus, Carlisle, Pennsylvania [7].
Wescott, Rev. Isaac, New York [8].
West, Charles E., Esq., New York [1].
Wetherill, Dr. Charles M., Philadelphia [4].
Wetherill, Prof. Leander, Rochester, New York [2].
Wethrell, Prof. L., Lagrange, Kentucky [2].
Weyman, G. W., Esq., Pittsburg, Pennsylvania [6].
Wheatland, Dr. Henry, Salem, Massachusetts [1].
Wheatley, Charles M., Esq., New York [1].
Whipple, W., Adrian, Michigan [7].
White, E. B., Esq., Charleston [3].
White, Rev. George, Marietta, Georgia [3].
Whiting, Col. H., Detroit, Michigan [1].
Whitman, William E., Esq., Philadelphia [1].
Whitney, Asa, Esq., Philadelphia [1].
Whitney, Eli, Esq., New Haven, Connecticut [4].
Whitney, J. D., Esq., Northampton, Massachusetts [1].
Whittemore, Thomas J., Esq., Cambridge, Massachusetts [2].
Whittich, —— ? Esq., Penfield, Georgia [3].
Whittlesey, Charles, Esq., Cleveland, Ohio [1].
Wightman, Rev. Dr., Charleston [3].

Wilcox, Lester, Esq., Canajoharie, New York [4].
Wilder, Henry, Esq., Lancaster, Massachusetts [1].
Wilder, L., Esq., Hoosick Falls, New York [1].
Wilkes, Capt. Charles, U. S. N., Washington, D. C. [1].
Williams, Abram E., Esq., Albany, New York [6].
Williams, Prof. Geo. P., Ann Arbor, Michigan [7].
Williams, Prof. L. D., Meadville, Pennsylvania [6].
Williams, Dr. P. O., Gouverneur, St. Lawrence Co., N. Y. [6].
Williams, Prof. S. K., Canonsburg, Pennsylvania [6].
Williman, Dr. A. B., Charleston [3].
Wilson, H. V., Esq., Cleveland, Ohio [6].
Wilson, Prof. James R., Hampden Sydney, Virginia [5].
Wilson, Prof. John, London, England [7].
Wilson, John, Esq., Staunton, Virginia [6].
Wilson, Hon. John Q., Albany, New York [6].
Wilson, Dr. Samuel, Charleston [3].
Wilson, Rev. William, Cincinnati, Ohio [2].
Winchell, Alexander, Esq., Eutaw, Alabama [3].
Wing, Dr. Joel, Albany, New York [4].
Wing, Matthew, Esq., Albany, New York [4].
Winlock, Prof. Joseph, Cambridge, Massachusetts [5].
Winslow, Rev. G., Staten Island, New York [1].
Winslow, Rufus K., Esq., Cleveland, Ohio [6].
Woodbridge, Rev. J. E., Boston [6].
*Woodbury, Hon. L., Portsmouth, New Hampshire [1].
Woodhull, Lieut. Maxwell, U. S. A., Washington, D. C. [4].
Woodrow, Prof. James, Milledgeville, Georgia [7].
Wool, Maj.-Gen. J. E., U. S. A., Troy, New York [6].
Woollett, William L., Esq., Albany, New York [6].
Woolsey, J. M., Esq., Cleveland, Ohio [6].
Woolsey, Pres. Theodore D., New Haven, Connecticut [4].
Worcester, Dr. Joseph E., Cambridge, Massachusetts [2].
Wormley, Theo. G., Columbus, Ohio [7].
Worther, A. H., Esq., Warsaw, Illinois [5].
Wragg, Dr. W., Charleston [3].
Wright, Albert B., Perrysburg, Ohio [7].
Wright, Charles W., Esq., Cincinnati, Ohio [5].
*Wright, Dr. John, Troy, New York [1].

Wright, J. J. B., M. D., U. S. A., Carlisle Barracks, Pa. [7].
Wynn, Dr., Baltimore [8].
Wynne, Thomas H., Richmond, Virginia [8].

Y.

Yandell, Prof. L. P., Louisville, Kentucky [5].
Yarnall, Prof. M., U. S. A. [8].
Yeadon, Richard, Esq., Charleston [3].
Youmans, E. L., Esq., Middlegrove, Saratoga Co., New York [6].
Young, Prof. Ira, Hanover, New Hampshire [7].
Young, J. A., M. D., Camden, South Carolina [7].
Young, William L., Esq., Vicksburg, Mississippi [5].

Z.

Zimmermann, C., Esq., Columbia, South Carolina [3].

MEMBERS ELECTED AT THE WASHINGTON MEETING.

Amory, Jonathan, Jamaica Plains, Mass.
Appleton, Thomas, Boston.
Badger, O. C., U. S. N.
Baer, Prof. William, Sykesville, Md.
Balestier, Joseph.
Barry, Rev. A. C., Racine, Wisc.
Bartlett, J. R., Providence, R. I.
Bassnett, Thomas, Ottawa, Ill.
Batchelder, J. M., Cambridge, Mass.
Bent, Lieut. Silas, U. S. N.
Berstow, William A., Madison, Wisc.
Browne, J. Blight, Juynedd, Penn.
Caldwell, L. W., Jr., Washington, D. C.
Canfield, Frederic, Tuckasunny, N. J.
Carey, H. C., Burlington, N. J.
Chandler, M. W. T., Philadelphia.
Chester, Prof. C., Rochester, N. Y.
Christie, David, Brantford, C. W.
Clark, Prof. P. P., Georgetown, D. C.
Clark, John H., Washington, D. C.
Colton, Willis S., New Haven, Conn.
Couch, Lieut. Darius N., Washington, D.C.
Curley, Prof. James, Georgetown, D. C.
Davis, W. M., Cincinnati, Ohio.
Dill, John B., Auburn, N. Y.
Dunglison, Dr. Robley, Philadelphia.
Eliot, C. W., Cambridge, Mass.
Evans, Walton W., Peru.
Ewbank, Thomas, Washington, D. C.
Farwell, Hon. L. J., Madison, Wisc.
Fearing, Dr. E. P., Nantucket, Mass.
Fisher, Thomas, Philadelphia.
Fitzgerald, W. P. N., Washington, D. C.
Foulke, William P., Philadelphia.
Gale, L. D., Washington, D. C.
Hamel, Dr., St. Petersburg, Russia.
Harlan, Prof. Joseph G., Haverford, Penn.
Harper, Prof. Lewis, Oxford, Miss.
Hedrick, Prof. B. S., Chapel Hill, N. C.
Hein, Samuel, Washington, D. C.
Herbert, Alfred, Washington, D. C.
Hilgard, Dr. T. C., Philadelphia, Penn.
Hopkins, J. A., Milwaukee, Wisc.
Hotchkiss, Gideon, Windsor, N. Y.
Hoyt, J. W., Cincinnati, Ohio.
Jacobs, Prof. M., Gettysburg, Penn.
Jones, Lieut. Catesby Ap. R., U. S. N.
Joy, Prof. C. A., Ovid, N. Y.
Kendall, David, Rochester, N. Y.
Kent, Edward N., New York.
Latham, Prof. Richard P., Richmond, Va.
Lesley, Joseph, Jr., Philadelphia.
Lesley, Dr. William W., Monticillo, Mo.
Lieber, Oscar M., Columbia, S. C.
Lund, Theodore D., Racine, Wisc.
Lyon, Hon. Caleb, Lyonsdale, N. Y.
Mac Cue, Maj. J. Marshall, Mt. Salon, Va.
McFarlan, Henry, Dover, N. J.
Macrae, Lieut. Archibald, N. C.
Mathiot, George, Washington, D. C.
Meech, C. W., Washington, D. C.
Möllhausen, ——, Berlin.
Moore, George H., New York.
Napoli, Prof. Raphael, Naples.
Norton, Rev. Niram, Meadville, Penn.
Opdyke, George, New York.
Osgood, Rev. Samuel, New York.
Owens, Dr. William, Cincinnati, Ohio.
Phelps, S. L., U. S. N., Washington, D. C.
Randell, John, New York.

Resor, Jacob, Cincinnati, Ohio.
Rhees, William J., Washington, D. C.
Richmond, A. D., Meadville, Penn.
Robinson, Charles D., Green Bay, Wisc.
Ruggles, Prof. William, Washington, D. C.
Russell, A. W., Washington, D. C.
Schenck, R. C., Ohio.
Schott, Arthur, Washington.
Schott, Charles A., Washington.
Sestini, Prof. Benedict, Georgetown, D. C.
Seymour, Rev. E., Bloomfield, N. J.
Shippen, William, Washington, D. C.
Smith, Capt. E. R., U. S. A.
Snow, Asa B., M. D., Lima, N. Y.
Snow, Charles B., Washington, D. C.
Spencer, Thomas, Philadelphia, Penn.
Starkweather, Judge, Cleveland, Ohio.
Stillman, Dr. C. H., Plainfield, N. J.
Stillman, Dr. J. D. B., New York.
Stillman, Thomas B., New York.
Sumner, George, Boston.
Swan, Lansing B., Rochester, N. Y.
Terlecki, Ignatius, Paris.
Tuthill, Franklin, M. D., New York.
Vail, Prof. Hugh, Haverford, Penn.
Wescott, Rev. Isaac, New York.
Wynn, Dr., Baltimore.
Wynne, Thomas H., Richmond, Va.
Yarnall, Prof. M., U. S. A.

NOTE. — This list contains ninety-nine names. It is said, on page 303, that eighty-two new members were elected at Washington. Others signed the Constitution, and became members without a formal election. This accounts for the larger number in the preceding list.

ADDRESS

OF

PROFESSOR BENJAMIN PEIRCE,

PRESIDENT OF THE AMERICAN ASSOCIATION FOR THE YEAR 1853,

ON RETIRING FROM THE DUTIES OF PRESIDENT.

MR. PRESIDENT AND GENTLEMEN OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE: —

In most offices, the duties terminate with the office, and the thing of the past, the ex-officer, is to the present an unknown quantity. But it is not so with your President. Science, with its time-annihilating power, which gives life to the fossil, which hurries the embryo future into premature birth, which ventures beyond the grave even to the foot of the invisible throne, sternly drags forward its reluctant presidents to their hardest trial when they have ceased to be, to a judgment after death severer than that of Rhadamanthus. This calling out of the actor upon the stage after the night of performance, when the blood is no longer warm, is all the worse to him who has never before made a set speech, all whose habits of thought are unknown to æsthetic display, and the Arctic latitudes of whose frigid studies are impenetrable to the God of eloquence and to the Muses who vibrate the silver-toned chords of human sympathy.

Geometry, to which I have devoted my life, is honored with the title of the Key of the Sciences; but it is the key of an ever-open door, which refuses to be shut, and through which the whole world is crowding, to make free, in unrestrained license, with the precious treasures within, thoughtless both of lock and key, of the door itself, and even of science, to which it owes such boundless possessions, this New World included. The door is wide open, and all may enter; but all do not enter with equal thoughtlessness. There are a few who wonder, as they approach, at the exhaustless wealth, as the sacred shepherd wondered at the burning bush of Horeb, which was ever burning and never consumed. Casting their shoes from off their feet, and the world's iron-shod doubts from their understanding, these children of the faithful take their first step upon the holy ground with reverential awe, and advance almost with timidity, fearful, as the signs of Deity break upon them, lest they shall be brought face to face with the Almighty. They are the searchers after truth, and do not pass the door or the key without careful scrutiny.

The key! It is of wonderful construction, with its infinity of combination, and its unlimited capacity to fit every lock, however varied in form and size; it closes the massive arches which guard the vaults whence the mechanic arts supply the warehouses of commerce, and it opens the minute cabinet in which the queen of the fairies protects her microscopic jewels; it is the great master-key, which unlocks every door of knowledge, and without which no discovery — no discovery which deserves the name, which is law and not isolated fact — has been or ever can be made. Fascinated by its symmetry, the geometer may, at times, have been too exclusively engrossed with his science, forgetful of its applications; he may have exalted it into his idol, and worshipped it; he may have degraded it into his toy, and childishly amused himself with the singular shapes which it would assume, when he should have been hard at work with it, using it for the benefit of mankind and the glory of his Creator. I have seen a watchmaker, who came into possession of a remarkable chronometer, which was made by a prince of the craft, — by one who only made a single chronometer in a year; but that single watch was a masterpiece of art, and in every part a model of exquisite workmanship. The single-hearted watchmaker would sit and gaze at the neat key of his chronometer by the hour together, wasting in admiration the precious time, which his faithful watch continued to measure. With the same simplicity of devotion, the mathematician, unable to resist her charms, may embrace his science too ardently, when it lies close to his heart; and thus her integrity may be suspected. But ascend with me above the dust, above the cloud, to the realms of the higher geometry, where the heavens are never obscured; where there is no impure vapor and no delusive or imperfect observation; where the new truths are already arisen, while they are yet dimly dawning upon the earth below; where the earth is a little planet; where the sun has dwindled to a star; where all the stars are lost in the Milky-Way to which they belong; where the Milky-Way is seen floating through space, like any other nebula; where the whole great girdle of the nebulæ has diminished to an atom, and has become as readily and completely submissive to the pen of the geometer, and the slave of his formula, as the single drop, which falls from the cloud, instinct with all the forces of the material world. Try with me the precision of measure with which the universe has been meted out; observe how exactly all the parts are fitted to the whole and to each other, and then declare who was present in the council-chamber when the Lord laid the foundations of the earth.

Begin with the heavens themselves; see how precisely the motions of the firmament have endured through the friction of the ages; observe the exactness of the revolutions of the stars; if these mighty orbs cannot resist the law, what can the atom do? Let, then, the resources of art be exhausted in this scrutiny. Let neither time nor labor nor money be spared. A slight defect of motion is just detected; it is slight, very slight, but it is unquestionable. We dare not hide it out of sight. Science must admit this triumph of art, and be true even if the stars are false. The names of fixed star and pole-star must not be suffered to impose upon the trusting world, and guide it in a delusive chase after an *ignis fatuus*. Geometry! to the rescue! Geometry is at her post, faithful among the faithless. The pen is at work, the midnight oil consumed, the magic circles drawn by the wise men of the East, and the wizard logarithm summoned from the North. The tables are turned. The defect of motion is transformed into the discovery of a new law. It becomes the proof of the atmosphere to bend the ray from its course as it shoots down, laden with the image of Arcturus and the sweet influences of the Pleiades; it becomes the proof of the moving light, of the unseen planet, and of the invisible star, and hence a new proof of the precision of the measure. Honor to Bradley, to Bessel, to Adams, and to Leverrier! The stars are not false. Question them as you may, they give the same evidence, and do not contradict each other's testimony. They tell us that ours is not the central sun, and that we are moving in the procession of the stars; they tell us that we move among the others, towards the constellation of Hercules, so that, while we grow in wisdom, we approach the strong man's home. They tell us that we are moving at such a rate, that the distance from star to star is but just a good geological day's journey; and hereby they confirm the story which is written upon the crust of our globe, and prove that the earth and the skies have been measured out with the same unit of measure.

Descend from the infinite to the infinitesimal. Long before Bacon and Galileo, before observation had begun to penetrate the veil under which Nature has hidden her mysteries, the restless mind sought some principle of power, strong enough, and of sufficient variety, to collect and bind together all the parts of a world. This seemed to be found, where one might least expect it, in abstract number. Everywhere the exactest numerical proportion was seen to constitute the spiritual element of the highest beauty. It was the harmony of music, and the music of song; the fastidious eye of the Athenian required the delicately curved outlines of the temple in which he worshipped his goddess to conform to the exact law of the hyperbola, and he traced the graceful features of her statue from the repulsive wrinkles of Arithmetic. Throughout nature, the omnipresent beautiful revealed an all-pervading language spoken to the human mind, and to man's highest capacity of comprehension. By whom was it spoken? Whether by the gods of the ocean and the land, by the ruling divinities of the sun, moon, and stars, or by the nymphs of the fountain and the dryads of the forest, it was one speech, and its written cipher was cabalistic. The cabala were those of number, and even if they transcended the gematric skill of the Rabbi and the hieroglyphical learning of the priest of Osiris, they were, distinctly and unmistakably, expressions of thought, uttered to mind by mind; they were the solutions of mathematical problems of extraordinary complexity. The bee of Hymettus solved its great problem of isoperimetry on the morning of creation; and the sword which threatened the life of Damocles vibrated the elliptic functions two thousand years before Legendre, Abel, and Jacobi had gained immortality by their discussion. The very spirits of the winds, when they were sent to carry the grateful harvest to the thirsting fields of Calabria, did not forget the geometry which they had studied in the caverns of Æolus, and of which the geologist is daily discovering their diagrams. When they traversed the forest, they vibrated the bending branches and the hanging vines into every variety of elastic and catenarian curve; as they passed over the city, they wreathed the rising smoke into spirals, at which the ancient philosopher could only gaze in admiration, as it ascended with double curvature in its lofty exponential path, and was lost to computation ere it vanished from sight; and even when, forgetting their beneficent mission, they raged, that awful night at sea, in a fearful struggle with gravitation for the dominion of the ocean, amidst the shrieks of the drowning sailor, they heaped up the waves into such majestic mountains, that the genius of the storm thundered his approbation, and man's analysis shrunk from the investigation of the strange forms, not daring even to give them a name.

Ancient philosophy, perceiving this power of number, did homage to it in all the simplicity, earnestness, and truthfulness which distinguished the early thinkers. Pythagoras and Plato, the founders of pure mathematics, turned their search inward to find in their own minds the origin of that force, which a universe of phenomena could only reveal in its effects, but not in its essence. They found there a principle, capable of ruling, restraining, and satisfying the extravagant fancy, the ardent imagination, and the licentious will, and of reducing all the faculties to harmonious and consistent action. Its dignified exterior was cold and forbidding, and marked with the gloomy inflexibility of the representative of justice, rather than with the gracious supremacy of a sovereign. Boldly penetrating it, they were rewarded with visions of sublime contemplation, such as the world had never yet beheld; and the majestic glories which surround the throne of number, to those few who are permitted to behold them, took their hearts captive. In the intensity of their enthusiasm, they unconsciously overstepped the bounds of human knowledge, and strove to grope their way where the torch of observation was not yet lighted. They sought in the monad, the duad, and the triad, the mysteries of the Divine nature; in the perfect number, the archetype of the highest good; and in such simple numerical ratios as their unaided reason could devise, the complicated logic of all life. The school-boy of the nineteenth century can detect and ridicule their errors, but the lovers of truth will always revere their memories, and the great discoverers will never cease to find in their magnificent investigations the elements of further progress. Ay! more than this! Modern science has realized some of the most fanciful of the Pythagorean and Platonic doctrines, and thereby justified the divinity of their spiritual instincts. Is it not significant of the nature of the creative intellect, the simplicity of the great laws of force? the fact that the same curious series of numbers is developed by the growing plant which assisted in marshalling the order of the planets? and that the marriage of the elements cannot be consummated except in strict accordance with the laws of definite proportion? This last extraordinary discovery has rescued chemistry for ever from the blighting thrall of superstition, and, elevating it to the rank of a science, has endowed it with the rudiments of a peculiar speech. The promise has just been given, by one of our own number, of a large extension of this fruitful law; Young America has given the pledge, and we have faith in her chivalry that she will redeem it; we may then hope that the atomic force will submit to some Newton of chemistry, and the formula of the crystal become as legible as that of the solar system. In all parts of the physical world, in sound and light, in electricity and magnetism, in the elements of the air and the ocean, the same precision is everywhere predominant. The tints of the morning cloud reflecting the smiles of Aurora, and the angry flash of the tempest, are equally exact expressions of the unwavering formula; and the geological Titans, sons of Vulcan and Neptune, who once piled Pelion upon Ossa and strewed the earth with the fragments of their battles, and more recently have arrayed the armies of science in unnatural conflict, are at length bound to the primitive rock of immutable law, by the same strong, embracing, golden chain of inductive argument, with which our Franklin "dragged the thunderer down to earth."

Every new discovery in science has now become a new conquest to geometry. Quantitative analysis is regarded as the only safe instrument of research, and the question of the "What kind?" is universally merged into that of the "How much?" This is not limited to the physical sciences; even in politics, the statesman, finding in each land all kinds of men and every element of public economy, is forced to inquire how much there is of each, and to be guided, in the conduct of government, by the figures of statistics. Can it, then, be otherwise than that the science which takes especial charge of the theory and laws of exact measurement should be of universal application? However distorted it may be in its technical forms, and diverted from its natural position by the injudicious zeal of its votaries, its fundamental principles are those of sound logic and good common-sense, and whatever it touches it elucidates and illuminates. There are many questions in which it might be advantageously consulted further than has yet been done, and it appears to me that the difficulty in regard to the claims to discovery would often be settled by its judicious application, although it might sometimes, perhaps, be tempted to divide an ill-begotten child with the sword of justice, so as to give each claimant his worthless share. But it would grant no countenance to that miserable spirit of scientific adventure, which, by a moderate fertility in suggesting possible solutions of an abstruse problem, lays the foundation for a claim adverse to the just rights of him who, by exact and profound investigation, has demonstrated the true doctrine. By the severity of the standard which it would establish, it might even compel science to renounce some of its pretended acquisitions. In Astronomy, for instance, it must be conceded that Saturn and Jupiter, Mars and Venus, are yet subject to unexplained irregularities of motion; that the theory of the asteroids has not advanced beyond the earliest stage of arithmetic; that the rings of Saturn are connected with their primary by a force not less mysterious than that which holds its golden representative upon the finger of the fair betrothed; and that the laws under which the tides obey the attractions of the sun and moon are quite undeveloped. The remarkable researches upon this subject made in the Coast Survey, have established that here still remains another world to be conquered, worthy the ambition of the Alexander of Geodesy.

There is, however, a broader basis than that of numerical accuracy for maintaining the central position of Geometry among the sciences. It is that of form; the grand type of structural combination. This element may often be deficient in the technical mathematician, but it is the characteristic feature of the imperial intellects of geometry, of Archimedes and Hipparchus, Newton and Leibnitz, Laplace and Lagrange, Monge and Gauss. It equally belongs to great ability in every department of knowledge and art, and directs all successful effort, from the brilliant campaign of the conqueror to the invention of the printing-press. It is the alpha and omega of intelligible speech, the architect of the poetic temple, the founder of empires, and the maker of constitutions. It is the power of combining innumerable details into a consistent whole, the highest exertion of human genius, and that which approaches nearest to the act of creation. It deciphers the hieroglyphic of events, and, uniting the present with the past and the future, it is the veracity of history and the inspiration of prophecy. It planned the vast fabric of the Reformation, and it touched with its miraculous finger the eyeballs of that statesman who foresaw, in its full development, the mighty tree which now overshadows this continent, when it was concentrated in the seed of liberty, and just germinating in the blood of the patriot. But with all its grandeur, this principle is subject to the laws of necessity and exactness, and when it ventures to build upon the sands of hypothesis and speculation, or the quicksands of *a priori* argument, the fall of its cathedral is certain, and only hastened by the weight of the massive towers. The rash system of philosophy which, despising the science it cannot comprehend, presumes to soar capriciously above the well-established theories of inductive demonstration, must melt its ill-cemented pinions as it approaches the source of truth, and sink, like Icarus, into deserved ridicule and contempt. The imagination of the immortal Kepler himself would have wasted all its strength in a wild and whimsical race with the mysteries of cosmography, if it had not been restrained from its extravagance by his sincere love of truth, and soberly harnessed to the observations of Tycho. The gaudy firmament of the artificial globe of the astronomer's studio is a singular illustration of the impossibility of devising a well-ordered plan, when there are no proper materials for classification and distribution. For more than twice the period of the millennium, the most savage beasts and horrible giants have been sporting upon it with infants and gentle maidens, and have clustered the stars in the mingled confusion of the wilderness and the nursery. The additions which modern taste and sycophancy have made to this curiosity-shop have not diminished its peculiar interest, increased its classic elegance, or relieved the perplexed interweaving of the constellations.

But the exactness required in the development of form is that of unity, order, and continuity, more than that of number; and it is better expressed in the curve than in the formula. Such accuracy may be developed in its highest perfection, in minds to which the processes of arithmetical computation are utterly distasteful. There is one, whom I am proud to call my friend, to whom I have more than once tried to communicate some conception of algebraic analysis and its modes of research. Whether the fault was in the obscurity of the teacher, or the too great density of the pupil's brain, my excess of modesty dares not decide. Whatever was the cause, the attempt was a total failure; I could not bring my friend to comprehend the product of two by two, when both the twos were negative; and I am firmly convinced that he would rather have yielded his fine teeth to the dentist, than his radical and absurd repugnance to the extraction of an impossible root. But of all men who ever set foot upon American soil, there is not one who has made so many and so great scientific discoveries as this man; there is not one who has opened so many new treasures of knowledge; and, paradoxical as it may seem, he has unlocked every door with the key of geometry. How was it, for instance, that he drew the outline of the fossil fish, from that of the single scale? and how did it happen that the original lithograph, when it was discovered, was identical with his design? When he was challenged before the British Association to portray the form of fish proper to a geological stratum in which he did not know that one was found, what power guided the hand which held the chalk? And when the cloth was removed, which, to his surprise, concealed the newly discovered fish, how and where came the extraordinary coincidence? and whence did they acquire the selfsame lineaments with the drawing upon the blackboard? To what other science than that of form is such a wonderful knowledge of form to be attributed? And are we not sorely tempted to confound all scientific distinctions, and claim him, even against his will, as one of the greatest of geometers, who has advanced the science far beyond the highest flight of the transcendental formula into the domain of the organic kingdom? When he was commissioned by the illustrious chief of the Coast Survey to examine the reefs of Florida, by what a consummate mathematical logic did he trace back their history for more than a thousand centuries, to the probable beginning of the present geological period!

Of the many difficult questions with which science is disturbed, none are so serious as those which are connected with religion. There are men, and pious men too, who seem honestly to think that science and religion are naturally opposed to each other; than which I cannot conceive a more monstrous absurdity. How can there be a more faithless species of infidelity, than to believe that the Deity has written his word upon the material universe and a contradiction of it in the Gospel? And is it possible that such a belief has ever been seriously professed? Or is the other alternative less unreasonable, that the serpent has wound its coils around the tree of knowledge, and that the alluring fruit which has been dropping at the foot of man from the days of Adam to those of Newton, is poisoned to its core? Shall we believe that the voices of Nature are the songs of the Siren and the artful temptations of the Devil, to divert man from his devotions? If this be so, how singularly forgetful has the Enemy been of his interests, or how divided against himself and his own household, that he is thus praising God and magnifying him for ever, "in all the works of creation; in the sun and moon, and the stars of heaven; in the showers and dew; in the winds; the fire and heat; the winter and summer; the light and darkness; the lightnings and clouds; the mountains and hills; and all the green things upon the earth"! that he is "for ever blessing the Lord in the whales and all that move in the waters; in the fowls of the air; and in the beasts and cattle"! Can it be that this universal anthem, this all-resounding chorus of hallelujah, is a cunning device of hell to gain the souls of men? and that the arch-hypocrite stole from the fire upon the tongues of the holy children whom he dared not touch? and that he has ventured behind the altar into the inmost sanctuary of the church, in the garb of a high-priest, and inscribed the sublime canticle upon the book of prayer, with the pen which was dipped in the blood of Christ? Or is it to be supposed that unconscious matter has abjured its Creator, and, having no will, has fallen with the infinite sin of man's rebellion, melting in the fires of its volcano the impress of the seal of approbation which was imprinted on the day of creation? Such absurd hypotheses may, in this age, be left to their own refutation.

There is proof enough furnished by every science, but by none more than by geometry, that the world to which we have been allotted is peculiarly adapted to our minds, and admirably fitted to promote our intellectual progress. There can be no reasonable doubt that it was part of the Creator's plan. How easily might the whole order have been transposed! How readily might we have been assigned to some complicated system which our feeble and finite powers could not have unravelled! to some one, perchance, of the stars, in the immense cluster of Hercules, where "the countless and unending orbs" are "in mazy motion intermingled," and where,

> "Above, below, around,
> The circling systems form
> A wilderness of harmony,"

the sound of which would never have penetrated to the desolate heart of man, nor have stirred his motionless spirit to a divine thought!

What a contrast to this humanly incomprehensible world is that in which we have our being, where the obvious simplicity and regularity of the daily phenomena invite our contemplation with so fine and irresistible a persuasion; where all the developments of art and nature occur in a happy and consistent gradation, ascending amid innumerable forms of beauty to the temple of science! Had it been the sole object of the physical universe to contribute to our intellectual nutriment, its arrangements could not have been more skilfully contrived. The angel of the Lord is ever sent with the message of new truth at the time when it is most welcome, when the heart of philosophy is heavy with the obscure mists which hang over the paths of knowledge, or when the faith of the fainting world requires the reviving influence of some brilliant discovery. How wonderful are the relations in the progress of the observing and the abstract sciences! With what a profound harmony do they sustain their several parts in the heaven-born symphony! Attracted by the symmetry of a few graceful curves, geometry undertook their investigation, and prosecuted the study of their curious and intricate properties, until ridicule threatened to point at such a useless expenditure of labor. But when, in the course of its revolving cycles, astronomy had grown to its grand era, it began by preaching to the astounded doctors of theology the amazing fact that these selfsame curves have been drawn, by the Creator's own hand, from the beginning of time, in the paths of the planet, the comet, and our own fixed earth; and thenceforth the treatise upon conic sections has become a chapter of the celestial Principia. And so it has ever been; in the ambitious flights to which geometry has been impelled by its impatience of the restraints of observation, it has never soared above the Almighty presence. And so it must ever be; the true thought of the created mind must have had its origin from the Creator; but with him thought is reality. It must then be that the loftiest conceptions of transcendental mathematics have been outwardly formed, in their complete expression and manifestation, in some region or other of the physical world; and that there must always be interwoven with the discoveries of observation these striking coincidences of human thought and nature's law. They are the reflections of the divine image of man's spirit from the clear surface of the eternal fountain of truth. Is then religion so false to God as to avert its face from science? Is the Church willing to declare a divorce of this holy marriage tie? Can she afford to renounce the external proofs of a God, having sympathy with man? Dare she excommunicate science, and answer at the judgment for the souls which are thus reluctantly compelled to infidelity? We reject the authority of the blind Scribes and Pharisees, who have hidden themselves from the light of heaven under such a darkness of bigotry. We claim our just rights and our share in the Church. The man of science is a man, and knows sin as much as other men, and equally with other men he needs the salvation of the Gospel. We acknowledge that the revelations of the physical world are addressed to the head, and do not minister to the wants of the heart; we acknowledge that science has no authority to interfere with the Scriptures and perplex the Holy Writ with forced and impossible constructions of language. This admission does not derogate from the dignity of science; and we claim that the sanctity of the Bible is equally undisturbed by the denial that it was endowed with authority over the truths of physical science. But we, nevertheless, as sons of men, claim our share in its messages of forgiveness, and will not be hindered of our inheritance by the unintelligible technicalities of sectarianism; as children, we kneel to the Church, and implore its sustenance, and entreat the constant aid and countenance of those great and good men who are its faithful servants and its surest support, whose presence and cheering sympathies are a perpetual benediction, and among whom shine the brightest lights of science as well as of religion. Moreover, as scientific men, we need the Bible to strengthen and confirm our faith in a supreme intellectual Power, to assure us that we are not imposing our forms of thought upon a fortuitous combination of dislocated atoms, but that we may study His works humbly, hopefully, and trusting that the treasury is not yet exhausted, but that there is still left an infinite vein of spiritual ore to be worked by American intellect.

Gentlemen of the American Association, I cannot conclude without a few words to recall our duty to the country to which we owe our allegiance. We must despise the base servility to foreign superiority, which affects to look down from the heights of the cosmopolite upon the duty of patriotism, and scorn it as an abomination. We must love our country with the same devoted, noble, and generous love which inspired the lives of Niebuhr and Arago, of Bowditch and our late lamented Walker, which is the living fountain of the labors of men like Humboldt, Henry, and Bache, which won for Franklin the affection, admiration, and reverence of France, and without which there can be no worthy respect and esteem for the labors of the men of other nations. The heart which is too small to hold its own country cannot assume to embrace the whole world, and still less to contain a science. Of all the virtues, patriotism is the least selfish, and that which is most kindred to the grand sentiments of the heroic soul. It repels foreign arrogance with dignified contempt, its proud spirit rejoices to do homage to true greatness wherever it may be found, and it frankly opens its hospitable door to the reception of the learned guest of every land. It was the larger half of the greatest name in history, and from the tomb of Washington it invokes us to be faithful to posterity.

The time is ripe for some important improvement in the public condition of science and its relations to government. For the first time in the history of the republic, "the men of genius of our country, who, by their inventions and discoveries in science and art, have contributed largely to the improvements of the age, without in many instances securing for themselves anything like an adequate reward," have been commended to the favorable consideration of Congress by our Chief Magistrate. For this great and good word, let the benedictions of science rest upon his head. It is now our fault if the occasion be not properly improved. No government, in proportion to its opportunities, has surpassed our own in its readiness to promote science. The Coast Survey, the National Observatory, the Nautical Almanac, the military and naval academies, the expeditions for astronomical and other scientific purposes, and the munificent grants for special researches, are conclusive evidence of the willingness to advance high and useful forms of philosophical inquiry. The confidence which has been inspired in other countries is shown in the trust of that magnificent endowment for the diffusion of knowledge to all mankind, under whose roof we are here assembled. The provisions for the election of the Board of Regents and the choice of a Secretary, upon the singleness of whose integrity and the largeness of whose comprehension are concentrated the hope and confidence of his scientific brethren throughout the world, are a guaranty that the honor of the republic will be held sacred in the discharge of this high charity, and that it will not be diverted into *any local channel* from the enlarged intentions of its testator. Let us profit by the example of Smithson, and, instructed by the wisdom of this high-minded son of England, learn to confide in our own rulers. Let us be aroused to an earnest and harmonious effort to accomplish the plan proposed by our President at Albany, for the building up of an "*institution for science, supplementary to existing institutions, to guide public action in reference to scientific matters.*" With the details of the plan and the arguments in its favor you are familiar. You know how useful it would be as a protection from the wasteful expenditure upon abortive attempts to reverse the laws of nature. You know how much it is required to sustain the purity and independence of science, even within its own proper domain. You know that in no age or country was there ever a more urgent call for a scientific society, in which scientific influence should predominate, where it should not be smothered by excess of patronage, and whence it should not be liable to banishment through any spirit or form of ostracism. If American genius is not fettered by the chains of necessity, and helplessly exposed to the assaults of envious mediocrity, but is generously nourished in the bosom of liberty, it will joyfully expand its free wings, and soar with the eagle to the conquest of the skies.

AN

ADDRESS

IN COMMEMORATION OF

SEARS COOK WALKER,

DELIVERED BEFORE THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, AT THEIR MEETING IN WASHINGTON CITY, APRIL 29, 1854.

BY BENJAMIN APTHORP GOULD, JR.

MR. PRESIDENT AND GENTLEMEN OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE: —

THE melancholy duty which you have imposed upon me is one which permits of no hesitation in its fulfilment. And though it is to be wished that the task had devolved upon an abler pen and a more eloquent tongue, your request has carried with it the force of a command, and at your bidding lips already too familiar with the funeral strain are here to utter the voice of our common lamentation and our common eulogy.

It is now more than fifteen months since all that was mortal of Sears Cook Walker was borne to its long home. Twice has his own planet Neptune, fitting emblem, disappeared in that solar effulgence which at once illuminates it and forbids it to our vision; — twice have the early flowers of spring blossomed above his honored grave. The places that knew him well know him no more, and the turmoil of life, like a great ocean, has rolled over the void that awed us by its depth and darkness. Those who called him their friend need no commemorative word, nor sound of eulogy.

But it is fitting that the votaries of science in America should unite, as such, to perpetuate the remembrance of his good works and pay to his memory the tribute of honor and gratitude which is its due.

No geographical division of our country may claim Walker as its own. Massachusetts was his birthplace; Pennsylvania long his home; this city the scene of his most brilliant achievements. The field of his investigations is bounded by Maine, Carolina, and Missouri, — and his ashes rest upon the banks of the Ohio. Where Science called him was his home; whom Science linked with him were his friends; for her he lived, and for her he died.

He was born on the 28th of March, 1805, in the little village of Wilmington, Mass., where his ancestors had lived and died for four successive generations. His father's mother was descended in a direct line from the celebrated Elder Brewster, who came out in the Mayflower. While yet an infant, he lost his father, but the care and devotion of an excellent mother guarded his childhood and youth, and directed his education with sound judgment and constant watchfulness. The activity and precocity of his intellect were extraordinary, and rendered him, at an early age, the wonder of the village. Few who knew him only in later years would have imagined that his constitution had in childhood been so feeble and delicate as to occasion the most anxious apprehension. Yet this was so, and for a time there was imminent danger that the slight tenement would prove too frail to sustain the restless activity which threatened to carry all before it. His judicious mother perceived the importance of providing first of all for his physical education, and of checking his intense application to books, but it was only with great difficulty that she succeeded. Still she was successful; she suppressed, at least in part, his devotion to study, and he joined for a time with all the zest of other children in the sports of his companions. Under this judicious guidance his health became gradually established, and after completing the preparatory studies at the Academies of Andover, Tyngsborough, and Billerica, he entered Harvard College in 1821, and graduated, four years later, a classmate of Horatio Greenough, Augustus A. Gould, and Charles H. Davis. While in college he was as conspicuous for his classical as for his mathematical attainments, and here, as throughout his subsequent life, manifested the peculiar fondness and aptitude for the acquisition of languages, and that singularly retentive memory which formed so prominent a feature of his intellectual constitution. For two years after his graduation he remained in the vicinity of Boston as teacher, and in the autumn of 1827 removed to Philadelphia, which continued for nearly twenty years his home and the seat of his constant labors. His was a choice combination of buoyant spirits and cheerful manners with an earnest, absorbing devotion to science, and in the refined, intellectual, and social city of his adoption he found a most congenial atmosphere. In Philadelphia he led the life of a teacher for eight years, and subsequently, from 1836 to 1845, was actuary of the Pennsylvania Company for the Insurance of Lives and Granting Annuities, a position analogous to that held in Boston at the same time by Bowditch.

As a teacher he was singularly gifted in the didactic art, and most successful in imbuing his pupils with zeal for science and letters. Many an ardent lover of nature, many an earnest votary of science, many a refined and elegant scholar, looks back with gratitude, affection, and reverence to those days when it was his privilege to imbibe intellectual refreshment from Walker's never-failing well of thought, and catch the enthusiasm which glowed in the soul of his departed teacher and friend.

In Philadelphia, the engrossing duties of his school, and subsequently of his office, left him comparatively little time for original investigation, even had the condition of the community in America been such as to promote this. Still his ever active intellect was ceaselessly at work, and the relatively less conspicuous efforts of his mind and pen — combined with that personal influence upon the public which every man of intellect is constantly exerting, whether voluntarily or not — were unquestionably of eminent service in paving the way for future progress, and in laying and cementing the basis on which we are striving to erect the structure of science in America. A physical frame singularly at variance with the restless activity of his intellect and with the fragile constitution of his early youth fettered his energies, and, for the period of uninterrupted prosperity and personal contentment, kept down the soarings of his genius and ambition. Still, even then, intellectual sloth was unknown to him. While engaged with his school he studied medicine, and went through the whole course requisite for the attainment of a degree. He devoted his leisure for a period to the study of Natural History, and was no mean proficient in Geology and Mineralogy, as well as in Physics and Chemistry. He was an active member of the Pennsylvania Geological Society, of the Committee of the Franklin Institute on Science and Art, and one of the most useful members of the American Philosophical Society. By frequent articles upon scientific topics in the various prints, by elaborate reports upon various subjects to the Franklin Institute, and by monthly announcements in its Journal of occultations and other important celestial phenomena, he kept awake the interest and sympathy of the community for studies of this character. Among other labors, he prepared, in 1834, an ingenious set of parallactic tables, by which the time required for computing the phases of an occultation was reduced to less than half an hour. These were calculated for the latitude of Philadelphia, and it was his intention to publish them in a more general form adapted to different latitudes. But as this would have been a work requiring considerable time, he subsequently abandoned the project, believing that he could employ his leisure hours more usefully. He continued the computation of the occultations without interruption for six years, and then induced our well-known colleague, Mr. Downes, to undertake the continuance of the work. It has been prosecuted to the present time, with what success we all know, and has of late years been published by the Smithsonian Institution and the Astronomical Ephemeris. Astronomy and Geography in America are much indebted to Mr. Walker for these labors, since many already in possession of the necessary means were stimulated by the periodical announcements, and by his personal exertions in still other ways, direct and indirect, to observe these phenomena. An extensive series of such observations was collected by Mr. Walker, and published in the Proceedings of the American Philosophical Society.*

Though Walker's position at this period was rather that of an amateur than of a scientist, — professed investigators being then with few exceptions unknown, if not unheard of, among us Cisatlantics, — and though his love for Nature in all her varied manifestations had not yet been concentrated into an absorbing devotion to any one department of science, Astronomy was always his favorite pursuit. He had procured an astronomical clock, a twenty-inch transit-instrument, and a small Dollond telescope, and the use of these gradually became so attractive and acquired such fascination for him, that he surrendered little by little the study of the other natural sciences, and from about 1836, in which year he gave up his school to become Actuary of the Insurance Company, all the leisure hours at his disposal were devoted to astronomical observation and study. In 1837 he was invited to propose a plan for an Observatory in connection with the Philadelphia High School, an invitation which he accepted with eagerness. In accordance with his suggestion, the committee in charge of the school imported from Munich the excellent Fraunhofer equatorial and Ertel meridian-circle, which, in his hands and those of his accomplished brother, the present Director of the Observatory, have done so much for Astronomy in America, — not merely by the number of observations made with them, but also by the incentive which they afforded to the lovers of Astronomy in other parts of the country. It is unquestionable, that in several instances they induced successful efforts for the procurement of similar, and even superior, apparatus elsewhere. Professor Loomis's Observatory at Hudson, Ohio, was erected at about the same period, and somewhat earlier provided with instruments, and these two were the first institutions in America deserving the name of observatories. The Philadelphia instruments were the first ones imported into this country from Germany, and the ripe judgment manifested by Walker in selecting the artists to whom their construction should be confided has borne abundant and valuable fruit. The constancy and value of his labors at the High-School Observatory, while oppressed by a multitude of other duties, are abundantly attested by the numerous observations and investigations which he published in the Proceedings and Transactions of the American Philosophical Society, and various journals. Besides the articles already referred to, Walker published in 1841 a most remarkable and brilliant paper concerning the periodical meteors of August and November; probably the most valuable memoir which has yet appeared upon that subject. Up to this time he may be looked upon as occupying a prominent position among American amateurs in astronomy. This article on meteors may be considered as first placing him in the ranks of scientific investigators.

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* Vol. I. New Series.

As we approach that eventful period in Walker's life which forms in fact the commencement of his real scientific career, in the higher sense, let me beg you to go back with me for about a dozen years, and consider the condition of science in the United States at that time, — say, for instance, in the year 1840.

The memoirs of our learned societies and academies, from the commencement of our national existence up to that epoch, furnished us with small authority for claiming a rank among the scientific nations of the world. Few, very few investigations of value had been made by Americans in any department of natural science, and what little had been done consisted, with but few, though indeed some brilliant exceptions, either of purely local observations, or of speculations, unrebuked however wild, unappreciated however profound. Our geology, botany, physical constants, even our topography and geodesy, had been examined and studied chiefly by men of foreign birth and education. The learning of the other hemisphere was wrapped in a kind of sacred mystery, deemed impenetrable by a native of these Occidental wilds. The very name of European carried with it a mighty weight of authority, and the favorable mention of an American name in a European print was a passport to that Notoriety which far and wide usurped the influence of Fame. The first lesson of the American scholar was, not self-reliance, but self-distrust, and intellectual timidity took the place of modest diffidence. There were, it is true, exceptions to these remarks, yet such as to render the general truth more conspicuous. I have but to point to the bright names of Hare, Bache, Henry, and Bowditch, and to the earnest devotion of Silliman. Yet it is not long since we have seen the possibility of the labors of these and similar men being set at naught for a while even in their own land, and by their own countrymen, as soon as a little foreign authority was brought to bear against them. Many circumstances tended to promote and protract this state of things. The want of proper libraries, a corresponding disregard of the importance of foreign languages, and a consequent want of acquaintance with the science of Continental Europe, except as presented in the garb of British translations and commentaries, all tended to delay the advent of an intellectual independence in any degree commensurate with that political independence which "our fathers fought for and bequeathed," two thirds of a century before. And all this dependence was largely aggravated by the deportment consistently maintained by the mother country, which, owing in part to the still lingering influences of colonial subjection and in part to the community of language, alone represented to us the science of the other hemisphere. The best efforts of the few devoted and self-sacrificing American scholars could obtain as their highest reward only some patronizing mention. Fortunate indeed was the inventor whose priority was acknowledged, — happy the experimenter or observer whose labors were noticed, though with disparagement. The combined action of these influences produced a reciprocal effect, which seemed almost to increase the evil already existing. Our ablest mathematician, whose genius would have rendered him a brilliant ornament of any scientific circle, was fully content that the crowning labor of his useful life should be a translation, enriched, it is true, with copious commentary, but a translation of the work of another. The most powerful and acute minds were content to exhaust their strength in the preparation of text-books. You, Mr. President and gentlemen, like all the votaries of science among us, are wont to contemplate with astonishment the remembrance of this period, at once so recent and so remote, — chronologically but as yesterday, and yet effaced from daily memory by the vast distance traversed since then in the onward progress of our nation. Most of the men whom we must thank for all this are still among us, full of honors, though but few of them, thank God, full of years. Walker has gone; fragrant be his memory.

Prominent among the producing causes which were silently but surely preparing the way for that vast step which was to transfer America from the rear to a prominent position in the army of nations combating for truth and the extension of the bounds of human knowledge, must be counted the noble enterprise of that good and venerable man who for so long a time and so ably has given a voice to American investigations in physics and natural history. The professedly scientific institutions of our country issued from time to time, though at considerable intervals, volumes of Transactions and Proceedings unquestionably not without their influence in keeping alive the scarcely kindled flame, but whose contents, as might be expected, were for the most part rather in conformity with the then existing standard of excellence than in advance of it. Natural History in the United States was the mere sorting of genera and species; the highest requisite for distinction in any physical science was the knowledge of what European students had attained; — Astronomy was in general confined to observations, and those not of the most refined character, and its merely descriptive departments were estimated far more highly than the study of its laws. Astronomical computation had hardly risen above the ciphering out of eclipses and occultations. Indeed, I risk nothing in saying that Astronomy had lost ground in America since those Colonial times when men like Rittenhouse kept up a constant scientific communication with students of Astronomy beyond the seas. And I believe I may farther say, that a single instance of a man's devoting himself to science as the only earthly guide, aim, and object of his life, while unassured of a professor's chair or some analogous appointment, upon which he might depend for subsistence, was utterly unknown.

Such was the state of science in general. In Astronomy the expensive appliances requisite for all observations of the higher class were wanting, and there was not in the United States, with the exception of the Hudson Observatory, to which Professor Loomis devoted such hours as he could spare from his duties in the College, a single establishment provided with the means of making an absolute determination of the place of any celestial body, or even relative determinations at all commensurate in accuracy with the demands of the times. The only instrument that could be thought of for the purpose was the Yale-College telescope, which, although provided with a micrometer, was destitute of the means of identifying comparison-stars. A better idea of American astronomy a dozen years ago can hardly be obtained than by quoting from an article published at that time by the eminent geometer who now retires from the position of President of this Association. He will forgive me the liberty, for the sake of the illustration. "The impossibility," said he, "of great national progress in Astronomy while the materials are for the most part imported can hardly need to be impressed upon the patrons of science in this country. . . . . . And next to the support of observers is the establishment of observatories. Something has been done for this purpose in various parts of the country, and it is earnestly to be hoped that the intimations which we have heard regarding the intentions of government may prove to be well founded; — that we shall soon have a permanent national observatory equal in its appointments to the best furnished ones of Europe; and that American ships will erelong calculate their longitudes and latitudes from an American nautical almanac. That there is on this side of the Atlantic a sufficient capacity for celestial observations is amply attested by the success which has attended the efforts, necessarily humble, which have hitherto been made." *

All good influences seemed to combine, at about this period, to inaugurate the new era. The discrimination and knowledge of Walker led him, in providing instruments for the High-School Observatory, to discard the class previously used in this country, and to equip the Philadelphia Observatory with the productions of German artists. This example was speedily followed, and within the next five years four noble Fraunhofer refractors were received and mounted in the United States. During these years the energy of our colleagues, Messrs. Bartlett, Gilliss, and Mitchel, equipped the fine Observatories of West Point, Washington, and Cincinnati; and the huge refractor at Cambridge was ordered within the same period. All things betokened the simultaneous devotion of earnest hearts and willing hands, and a concentration of their effort upon the work of advancing Astronomy in the United States; and it was precisely at this period, fortunately for us and for the renown of our departed colleague, that a crisis in his life occurred.

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* Peirce, Cambridge Miscellany, April, 1842, p. 25.

A series of unfortunate investments and commercial operations led to most disastrous results, and left Mr. Walker at the age of forty years utterly without means. It was a bitter cup for a man accustomed to the luxuries of life, and all the little comforts of opulence, which none knew how to prize better than he; but that Providence which had prepared for him so severe a trial

> "Knew how to bring honey out of the lion."

Often since that eventful time have I heard him bless the dispensation, which first made manifest to himself and to mankind the rare endowments of his genius and the exhaustless energies of his intellect.

Under these circumstances he was invited by the Secretary of the Navy, in 1845, to take part in the Washington Observatory; an invitation which, it is needless to say, he accepted with alacrity. Here he found himself for the first time in his life surrounded with all the facilities which he desired, and he applied himself with energy to the new field of duty open before him. But circumstances which it is not necessary to detail led him to resign his post at the Observatory to accept the direction of the Longitude department of the Coast-Survey; — an office which he filled until his last illness, with honor to himself and credit to the national service, as well as to American Astronomy.

During the eight years of Walker's residence in Washington, science in America was developed with almost electric rapidity. Scientific institutions and men sprang up, as it were by magic, in every section of the United States. The Coast-Survey under its present honored Chief entered upon new and enlarged activity. The Smithsonian Institution was organized. This Association was formed, and increased life infused into every department of research. It is our high privilege, Mr. President and gentlemen, that our lot has been cast in such a time, and that we have been permitted to act together in behalf of such a cause. It is unquestionable that the latent energies which imparted this impulse to all scientific investigation, and were prominently manifested in Astronomy, aided to throw the important agency of Walker into bold relief. But it is no less true that he whom we here lament bore an important part in the great work, and that to his boundless enthusiasm, his genius, and his well-directed labors, the nation's warmest gratitude is due.

During these years of his residence in Washington, Mr. Walker made many investigations, and on many subjects; but the first, and that which brought him most prominently before the scientific world, was his series of researches connected with the planet Neptune. The remarkable combination of singular and imposing events attending the discovery of the planet and the determination of its orbit needs no repetition here. On the 2d of February, 1847, but little more than four months after the discovery of Neptune, Mr. Walker, without the aid of charts or telescopes, in the solitude of his chamber, made the discovery that a star observed by Lalande in May, 1795, must have been the same planet. The telescope of yonder Observatory, in the hands of our colleague, Professor Hubbard, confirmed the bold prediction on the first clear evening, and astronomers were thus furnished with an observation of Neptune made fifty-two years before, which afforded the means of a most accurate determination of the orbit. This discovery of Walker's was followed, at an interval of but few weeks, by an independent discovery in Europe of the same important fact by means of a totally different method; — viz. a laborious examination of the heavens through a zone 6° broad and 45° long, and a careful comparison of this region with the observations of Lalande.

Gentlemen, Astronomy is now also deploring the afflicting bereavement by which we have been deprived of the companionship and aid of that earnest, self-sacrificing, devoted man to whom this European discovery was due, — a man whose singleness of heart was only equalled by his truthfulness and zeal for science. Three months only have elapsed since the death of Petersen. Forgive me if I pause to ask your tribute to his memory. He was a man whose warm affections and large soul were utterly unknowing of the petty rivalries and dishonoring jealousies which, alas! too often, in all countries and all times, furnish a painful evidence that Science alone, without the influence of some higher guide and aim, is insufficient to elevate her followers above the level of the world's turmoil and passions and cares. He was an ardent friend, a generous colleague, an enthusiastic scientist. Let me apply both to Walker and to Petersen the words which Hansen dedicated to the latter. "Hail to the man who can die as well as live for his vocation! A glorious death is his, and his name shall shine bright in never-dimming lustre." Gentlemen, if all ages and realms have joined in echoing the "*Dulce et decorum est pro patria mori*," when that death is amid the maddening shouts of the battle-field, — when the effulgent hopes of victory and tokens of a nation's gratitude and honor blind the sight to the uncertain dangers in the way, — what should be said of the enthusiastic men who calmly and quietly, often, like Petersen and Walker, with all the self-devotion, though without the external encouragement, of a forlorn hope, lay down their lives for science, truth, and their country's fame?

The discovery of the identity of Neptune and the star of Lalande was confirmed in a remarkable manner by the examination of Lalande's manuscripts in Paris, and Walker devoted himself with ardor to the determination of the orbit. Meantime, Peirce had made his investigations of the theory of Neptune, and his discovery of the two solutions of the problem, and of that singular coincidence of direction between the computed position of the hypothetical planet at the time of discovery and the observed position of Neptune, — a coincidence doubtless the most wonderful in the history of astronomy, and not by any means less striking from the fact that the coincidence was not in place also as well as in direction. Walker's orbit furnished Peirce with the materials for a still more thorough investigation of the theory and redetermination of the perturbations. Peirce's perturbations gave Walker the means of attaining an orbit yet more rigorously exact, which Peirce used again in his turn; and thus, by the joint labors of our two American astronomers, the theory of Neptune was placed, within eighteen months after the discovery of the planet, upon so sure and accurate a basis, that we risk nothing in asserting that the conformity between the predicted and observed places is far more close for Neptune than for any other planet in the heavens. This was of course a possibility only by reason of the minute eccentricity of the orbit, which alone rendered such an achievement feasible; but it is one of the most brilliant specimens of successful numerical computation on record. And two years later the planet was found so near to the predicted place, that, had another planet existed to mark the place predicted by Walker, it would have formed with Neptune a double star so close that few existing telescopes would have been able to recognize a dark line between.

I pass from this brilliant achievement to another equally brilliant, and which has done yet more to register him as "one of the few, the immortal names that were not born to die." I allude, of course, to the arrangement and development of the American method of observation.

It is impossible, Mr. President and Gentlemen, not to be aware that I am here treading upon dangerous ground. But I will not be false to the memory of my departed friend; and while I trust that the reluctance with which I enter on a subject from around which the clouds of contentious rivalry have not yet wholly cleared away, and that an earnest, faithful, and impartial study of the facts may shield me from any imputation of injustice, the memory of Walker demands, and shall receive, the honor which is its due.

Soon after the completion of the first line of telegraph in this country, — that between Washington and Baltimore, — the means thus offered for the determination of the difference of longitude of these two places was made use of by Captain Wilkes. The correction of a chronometer was determined at each end by transit-observations, and these were compared by telegraphic signals. This method, though far from possessing the precision requisite for geodetic purposes, was amply sufficient for the end desired at the time; and the experiment is historically interesting, as the first recorded telegraphic determination of a difference of longitude.

In the autumn of 1845, two or three months before Walker left Philadelphia for Washington, Professor Bache, who had but recently assumed the direction of the Coast-Survey, entered into communication with Mr. Walker regarding the telegraphic determination of longitude, and requested him to make arrangements in behalf of the Coast-Survey with the Telegraph Companies for the use of their lines. Negotiations for this purpose commenced in January, 1846, and, though temporarily interrupted by Walker's removal to Washington, and assumption of duties at the Observatory, in February, were only delayed for a time. Before his change of residence, the subject was thoroughly discussed between Messrs. Bache and Walker, and the important step of substituting transits of stars for arbitrary signals coinciding with the beats of a time-keeper, was determined on. Mr. Walker has informed me that this suggestion was due to the Superintendent of the Survey; but its practical application seems to have been a result in the elaboration of which the two bore an equal part. At least I may be permitted to state the still more honorable fact, that, in the very many conversations which it has been my privilege to hold with each of the two gentlemen separately upon this interesting question, their descriptions varied but in one salient point, namely, that each ascribed the chief merit to the other.

In a letter to Professor Bache, dated October 3, 1846, the mathematical theory of the subject was fully investigated, and lithographed circulars of instruction issued for the guidance of assistants; and on the 10th of the same month the transit of a star was telegraphed to Philadelphia by Lieutenant Almy of the Navy, then attached to the Observatory and now to the Coast-Survey. "This," to use Walker's own words to this Association, in 1849, "was the first practical application of the method of star-signals, which is destined sooner or later to perfect the geography of the globe."

Sundry influences combining to embarrass farther proceedings, the work was postponed until the following year, when Mr. Walker entered the Coast-Survey service as a regular officer, and renewed the prosecution of these operations with vigor. On the 27th of July, 1847, he introduced another most important improvement by applying the method of coincidence of beats to the comparison of time-keepers at the two ends; the one indicating mean and the other sidereal time. These beats were signalized from one station to the other by taps of the observer upon the telegraph-key, as nearly as possible synchronous with the ticks of the clock, and forming a graduated register of time upon the running telegraph fillet; a "personal visible register," as Walker called it. The problem which next presented itself was to form a satisfactory "automatic visible register," that is to say, to connect an astronomical clock with a telegraphic circuit, in such a manner that the clock should give its own signals, and the error inseparable from the transference of the signals from the clock to the telegraph through the agency of personal perception and volition thus entirely avoided. This end had indeed been attained by apparatus suggested and used by Mr. Saxton for the measurement of short intervals of time. In 1843, at the launch of the frigate Raritan, he had employed an electrotome, consisting of a light tilt-hammer of platinum placed opposite the centre of percussion of the pendulum, and struck by it at each oscillation, — and in 1846 had already suggested the use of a globule of quicksilver placed under the extremity of the pendulum in such a position as to form a contact at the instant of verticality. The entire avoidance of any method by which the rate of the clock could possibly be affected was so indispensable, that it was at first deemed hazardous to employ either of these methods, since the supposed jar to the pendulum in the one case, and the passage of the current through the pendulum-rod in the other, exposed the apparatus to criticism on theoretical grounds. It is, however, but just to state, in this connection, that no practical inconvenience is found to result from either of these features, — a fact abundantly attested by the circumstance that the former of them is at this moment the only method used in the Coast-Survey and the Observatory at Cambridge, and the latter, with some ingenious modifications by Professor Keith, has been adopted at the Washington Observatory.

Mr. Walker's high standard of accuracy and precision led him to spare no exertion to obtain some apparatus which should be free even from theoretical objection; and during the subsequent summer he propounded the problem to Mr. Bond, at Cambridge, and to Professors Mitchel and Locke, at Cincinnati. Each of these gentlemen proposed a method, and constructed an apparatus for the purpose; that of our ingenious colleague, the astronomer of Cincinnati, having been put in operation and exhibited on the next day after the problem was submitted to him. This is not the appropriate occasion for any discussion of the merits of the methods proposed. To do less than assert that the mechanical problem, with a view to its astronomical and geodetic applications, was distinctly propounded by Walker, and by Walker alone, would be unpardonable. And although he might, as regards the public and tangible expression of a nation's gratitude, have written, like Virgil,

> "Hos ego feci, — tulit alter honores,"

he may also share the triumph of the Mantuan, by the decisive, even if later, public vindication of his deserts.

The apparatus thus constructed furnished a graduated scale on which the commencement of every second was distinctly marked, so that, when connected with signal-keys, the instant of the occurrence of any phenomenon might be recorded upon the same scale with sufficient precision to permit of the accurate measurement of time to within a few hundredths of a second.

It became manifest that the gain was not for the determination of longitudes alone, but for astronomical observations of every kind requiring minute precision in the determination of time. Estimate, gentlemen, for yourselves the importance of this step to Practical Astronomy. Walker immediately modified the transit-instrument to suit the new requisitions, and instead of five, seven, or, at the most, nine threads, he provided it with a number of tallies of five threads each. The skill of our accomplished artist, Mr. Wurdemann, has brought the construction of these diaphragms to a high degree of precision, and equipped with them many instruments of the Coast-Survey and of several observatories. No mental effort is requisite for an observation by the new method, — no careful attention to the clock, no counting of the seconds from the low ticks of the pendulum, — but all the attention and care of the observer are concentrated upon the accurate perception of the moment of passage across each thread, and as his fingers rest lightly on the key, their almost involuntary muscular contraction at the instant of transit gives the desired signal.

There remained apparently but one step more to complete the American method of observation, the mechanical attainment of a rotary motion, uniform, from second to second, within the limits of the unavoidable errors of our measurement. Unless this be attained, the measured space upon the record is not strictly proportionate to the elapsed time. This problem, although it has enlisted the attention of able minds, is not yet wholly solved. The ingenuity of Saxton, Bond, and Kerrison has been enlisted in the cause, and year after year, as our Association assembles in its annual round, we are gladdened by the announcement that one difficulty after another has been surmounted by the assiduity and genius of our gifted colleague of Cincinnati. The improvements which Professor Mitchel announced at the Cleveland meeting have brought us very much nearer the desired goal; and the fact that his interest is still engaged in the enterprise is an earnest of future, and probably not distant, success. During Walker's residence in Cincinnati, soon after leaving Trenton, and but a short time before his death, he visited the Observatory, and Professor Mitchel exhibited the improvements he had already introduced in the apparatus, and explained those which he had in contemplation. Would that Walker might have witnessed their complete triumph, — the crowning glory of his achievement!

At a single meridian passage twenty-five transits are now observed with less fatigue than was formerly required for seven, and in the same time. Each of these transits is worth at least four by the old method, so that the relative efficiency of the two modes of observing is as 100 to 7. This is a low estimate, and derived from the experience of three or four years.

The telegraphic observations between Cambridge, New York, Philadelphia, and Washington furnished a singular and most unexpected result, and one which could not escape the acute perception of Mr. Walker. It appeared not only that the time required for the transmission of the electro-telegraphic signals was appreciable, but that it was — under the circumstances at least in which his longitude-observations had been made — less than a tenth part of the velocity of light in planetary space. This result was so extraordinary, and so greatly at variance with the previous ideas, that acquiescence in it was for some time refused, and it was not until the celebrated velocity experiments between St. Louis and Washington that the reliability of his results was put beyond question. Some European physicists, indeed, still refuse their credence, and experiments made under different circumstances still yield apparently widely diverse results. The present activity of investigation will doubtless soon conduct us to a thorough understanding of all the modifying influences. But Walker's claim to the brilliant discovery cannot be controverted; nor can the fact, that it was himself who stimulated and encouraged the investigations of others. And with a nobleness of soul which those who knew him best can best understand, he aided and sympathized with such researches when they appeared to conflict with his own results, as cordially as when they furnished his theories with confirmation.

The proper limits of this address preclude me from entering more especially upon the description of the works of the great man whose loss we mourn. Let it not be thought, because I have specified these three more brilliant exploits alone, that these were all, or the chief part, of his honorable labors. Numerous able reports and accurate researches enrich the records of the Coast-Survey, and adorn the pages of scientific periodicals on both sides of the Atlantic.

There is one more of his achievements to which I must allude, even though briefly. It is well known to the astronomical portion of this Association, that in the English Nautical Almanac for 1856 appeared (in 1853) a profound, elaborate, and most valuable paper by Mr. Adams, in which this eminent mathematician and astronomer discusses with his characteristic ability the amount of the lunar parallax. The tables of Burckhardt have, until recently, been the standard ones, but Adams shows in this paper that, besides a constant correction, the value of which he has deduced with care, there are a number of important terms to be added to Burckhardt's value, these combined errors sometimes amounting to as large a quantity as 6″. In concluding these masterly researches, Adams alludes to the important effect which such errors in the lunar parallax may exert upon the determination of longitudes from occultations, and expresses the opinion, that a great part of the observed difference in values derived from different occultations should be attributed to the use of an erroneous value for the parallax.

Gentlemen, you will in the course of this session learn from the Superintendent of the Coast-Survey, that our Walker, in April, 1848, presented a report on longitudes, in which, after showing that an error must exist in these longitude-determinations of nearly the same order in seconds of time as the error of the lunar parallax in seconds of arc, he proved that such an error did actually exist, and proceeded to discuss the tables of Burckhardt. In this discussion (presented to the House of Representatives, December 18, 1848, and printed with Exec. Doc. No. 13, XXX. Congr. 2d Session) Walker points out the chief errors in the tables, and shows that the resulting error in previous determinations of the chief points of this hemisphere, as counted from a prime meridian in the other, amounted to two seconds of time. His estimate of the constant error of Burckhardt amounts to nearly the same as Adams's new computation; and he has given four out of the five principal terms with a precision truly astonishing. The sum of the coefficients of the terms indicated by Walker is two thirds of the sum of those of all the terms given by Adams, and our lamented countryman anticipated the distinguished astronomer of England by more than four years.

The additions to the world's science which I have described might either of them justly claim the world's homage and gratitude to his memory, and it may be pardonable if we Americans claim a peculiar share in his renown, not merely because he was our own, but because his most valuable labors were in the national service, as an officer of the United States Coast-Survey. Quoting his own words to this Association, when speaking of the new, American method of observation, "Since this operation is likely to come into general use in geography, geodesy, and hydrography, the origin of the method must be a subject of historical interest. ..... As respects the service in which we are engaged, this origin is a matter of importance. It is a subject of just pride to those engaged in it, that the first conception of the method, and the first practical operation with it, are peculiarly its own. ..... I deem it, therefore, a duty to declare, that, with the single exception of the experiment between Baltimore and Washington, by Captain Wilkes, in 1844, I know of no telegraphic operation for longitude, and of no step in the improvement or perfectionment of the art, in Europe or America, which has not been the work of the officers proper of the Coast-Survey, or of commissioned officers and civilians acting temporarily as assistants. . . . . . I will not here allude to the respective claims of Americans for priority or superior excellence of inventions and suggestions, believing that it will be becoming for all of us to look to the great work that has been accomplished by our united efforts, rather than to the single share of each."

That last sentence, Mr. President and Gentlemen, would alone be a monument to his memory.

It is an interesting fact, too, that Walker's labors concerning Neptune were not only stimulated, and in a measure prompted, by the institution within whose walls we are assembled, but that they were, if I mistake not, the first of its publications.

It is not only a token of vitiated taste, but, more than that, it is unworthy of seekers after truth, that the narrow boundaries of terrestrial geography and the small peculiarities of nationality should, as a general thing, enter prominently into our judgment, or even be inseparably associated in our minds with those great discoveries or advances which are the property of our race. It is debasing to those who aspire to the lofty calling of priests in that great catholic temple of Science, where the embodied thoughts of the Most High are revealed in all their majesty and glory to mortal man, that they should descend to unworthy rivalries and discordant claims, as to who first heard the Voice, or who first invoked the sacred Presence. Far from us be all such impulses banished.

But the most earnest patriotism is by no means inconsistent with the most disinterested devotion to Science. And while we deprecate the unworthiness of contests for mere priority, or that national boastfulness which would extol itself at the expense of allies in the great crusade, we may be permitted to gather hopefulness and courage from past success, and to cultivate that peaceful competition which springs from mutual emulation in a noble cause. Walker's condemnation of the spirit which would permit an American in the present condition of our science to send his researches across the Atlantic for publication, when the means for their publication here was at hand, was constant and deep. And no enterprise which even in the most indirect way would aid in the development of scientific taste or culture, or the elevation of the standard of excellence, wanted his hearty and constant aid, not in words merely, but in solid, effectual deeds. It is thus that Walker was patriotic; it is thus that I invoke your pride as Americans. He was one of the foremost champions in the great struggle in which we are all now enlisted for the advancement of science by means of its advancement in America; and when, from failing health or exhausted energies, all other motives had spent their force, the thought of his country's honor has nerved him again to the task. I have seen his eye flash, and his wearied features light up, at the thought that his aching head and overstrained nerves were suffering for his country's glory; and when prostrate on a bed of pain, his was the enthusiasm the wounded patriot feels in the moment of victory.

I have said that the crisis which fully developed Walker's character and powers was opportune for him and for us. Just at the moment when the feeble science of America most needed willing hands and able minds and earnest hearts, to protect, to guide and impel, to battle with her most insidious foe, that avid tribe which not only claims her kindred without title, but would fain make her, as Schiller says, their cow and not their goddess, — just as, in the progress of our national civilization, institutions were founding in all quarters of the land, which, in the absence of proper guidance, may easily prove as fatal as they were designed to be propitious, — just as the efforts of good men were combining in all quarters of the land for the great cause they had so deeply at heart, — Walker came forward, and labored for his country, for truth, and for mankind. To him belongs a prominent place among the founders of Science in America. And if I lay stress on the rank which his labors have aided our country to assume, it is because I would claim just ground for pride in America's recent progress and her present tendency, rather than for the aggregate of her contributions thus far to the science of the world.

It was Walker's custom, during the last five years of his life, to escape from the heat of Washington, and pass the summer months in Cambridge. But neither the temptations of social intercourse with his numerous friends of kindred taste residing there, nor the close vicinage of the ocean he so much loved, nor yet the charms of the scenes of his youth, could wean him from his constant, unceasing mental application. Here it was that, in August, 1851, he was visited with a slight attack of paralysis, which for a few days deprived him of the use of one hand. Still he would not leave his investigations. The warnings of friends, the entreaties of relatives, the appeals of the physician, were all in vain; he could not abandon his studies and cherished pursuits. The dawn surprised him at his labors, and the eastern sunlight scarcely touched his pillow before he was again at work.

Yet the attack and its effects appeared to pass away, and during the same autumn Mr. Walker took charge of the telegraphic expedition at Bangor, Maine, for determining the differences of longitude between Halifax, Bangor, and Cambridge, — an expedition which was hardly terminated before the beginning of the year 1852. He had been but two days in Washington, on his return, before symptoms of mental alienation appeared, the true character of which was but too evident. The energy of the imprisoned intellect was disproportionate to the walls of flesh, and even the malady still illustrated the wondrous activity and power of his mind, and the wide scope of his learning. He remained a short time in the asylum at Mount Hope, near Baltimore, and thence, in April, 1852, was transferred to Trenton, New Jersey, where in the skilful and judicious Superintendent, Dr. Buttolph, he found at once a wise counsellor and a sympathizing friend. Those who loved Walker owe to this gifted and true-hearted man a debt of gratitude and respect which time cannot efface.

In the beautiful seclusion of Trenton, Walker's mind reassumed by degrees its normal condition. Frequent visits of friends and the companionship of a devoted sister were permitted him; he corresponded with friends at a distance, who kept him informed of the progress of astronomical researches and discoveries, and the earnest, touching appeals for his books and papers were gradually complied with. Here he computed the ephemeris of Neptune published in the American Astronomical Ephemeris for 1855, and in the autumn of the same year left the institution with apparently sound mind and sound body; though of course greatly debilitated by the effects of his illness, and physically hardly the shadow of his former self.

He immediately visited his brother, Hon. Timothy Walker, in Cincinnati, with the intention of remaining there until the early spring. His letters from that city show him the same that he was of old, — the enthusiastic scientist, the warm-hearted friend. He resumed his labors for the Coast-Survey, and arranged for the renewal of his former sphere of activity, re-engaging his apartments, and fixing the day for his journey. But, alas! he was never again to look upon the blue waters of Potomac, nor the proud dome of the Capitol. An attack of fever, at first considered merely as a slight indisposition, proved too much for his enfeebled frame to withstand. Other maladies followed, and on the 30th of January, 1853, Walker died, — but in the midst of his friends and kindred. His ashes rest in the cemetery of Spring Grove, some four miles north of Cincinnati, on the banks of a beautiful tributary of the Ohio, called by the Indians *Mahkatewa.*

Mr. President and Gentlemen, you know how the Science of America has mourned for our departed Walker. Of her deep bereavement it is needless for me to speak. She was his only love. To her he had given all the devotion of a great and earnest heart, and he had experienced the ennobling influences of such devotion.

> "His spirit, ere our fatal loss
> Did ever rise from high to higher;
> As mounts the heavenward altar-fire,
> As flies the lighter through the gross."

The most conspicuous attribute of Walker's mental character was his intellectual activity. In despite of the serious obstacle offered by his physical constitution, — an obstacle which he felt most keenly, and which, indeed, prevented him from obtaining the bodily exercise imperative for the intellectual as well as corporeal health of the student, his ever vigorous mind was ceaselessly at work. Intellectual labor was the atmosphere he breathed, — he only lived in work. Never idle, the love of science was the moving spring of every deed and thought and aspiration, and the approval of those whose judgment was entitled to confidence was the highest reward he asked. Impulsive to a fault, his impulses were ever for some noble cause. And the liberality with which his mind was always open to conviction, the magnanimity with which he was at all times ready to acknowledge an error, and a peculiar readiness to overlook even injustice and moral error in others, were among the most prominent of his traits. That he was free from faults, no one would claim; — who is? Honor to the man whose failings are the offspring of generous impulses, whose weaknesses testify to the generosity of his heart, and of whom we may be sure, even if he err, that he has erred in order that, in his opinion, good might come. The transparency and childlike simplicity of his character misled those to whom such simplicity was foreign, and after seeking in vain to detect some hidden motive, they attributed to him a profound tact and a shrewdness utterly at variance with his nature. A child of Genius, he followed whither his nature led, and all the outward bustle of the world had for him a less than secondary importance. Neither fitted nor inclined for a life of active participation in the world's affairs, yet gifted pre-eminently with the spirit of social communion and joyous companionship, he was a cheerful, yes, a happy man, even under depressing influences; and when the clouds of earth obscured and darkened his way, he could soar above them to the pure expanse of the intellectual empyrean.

> "He whom thou never leavest, Genius,
> Feels no dread arise within his heart
> At the tempest or the rain.
> Thou wilt place him on thy fleecy pinion
> When he sleepeth on the rock ;—
> Thou wilt shelter with thy guardian wing,
> In the forest's midnight hour."

I should do wrong did I not allude to the classical attainments of Mr. Walker, which were great and varied. They were the solace of his weariness, and an unfailing well of constant pleasure. He possessed a singularly retentive memory for poetry. He could remember, and was wont to repeat, long passages from the Greek, Latin, and Italian poets. Tasso, his especial favorite, he knew by heart. Although his tastes, his intellect, his very affections, were devoted to the study of the exact sciences, he was no stranger to the refining and genial influences of classic culture. None were more earnest than he in combating the hazardous theory which is unfortunately so widely disseminated in the United States, that classical studies have hitherto occupied an undue prominence in the education of youth. His first advice to young men was to acquire a thorough knowledge of ancient and modern languages. When, some nine years ago, it was first my privilege to know Mr. Walker, I went to him, just after leaving college, to ask his advice as to future studies, and the best means of attaining scientific usefulness. "Study foreign languages," was his advice ; "for thus alone can you keep pace with the progress of modern science. Read Laplace, Lagrange, Gauss, Bessel, Plana, in their own tongues, and accustom yourself to think with them in their own words." It was thus that Walker attained his pre-eminent position, it is thus that those who aspire to emulate his bright example would do well to direct their energies.

Among the number, happily not small, of the great men whose joy it has been to extend the guiding, helping, and protecting hand to young men aspiring to pursue a life of scientific usefulness, none surpassed Walker in this noble trait. Who was so ready to aid? who so eager to assist? in doubt and obscurity, who could guide so well as he?

He is gone. That helping, welcoming hand is palsied in the grave. That earnest voice is heard no longer in our midst. That powerful and restless intellect gives token of itself no longer. The eye that tracked the distant Neptune through the wilderness of space is closed for ever. The strong will that subdued all the forces of Nature to his own loved science is broken. And the quick thought that outstripped the electric messenger, and measured his pace, is stilled for evermore. He is gone. His memory alone remains, his noble memory.

" He was our own. How social, yet how great
Seemed in the light of day his noble mind!
How was his nature pleasing yet sedate!
Now for glad converse joyously inclined,
Then, swiftly changing, spirit-fraught, elate
Heaven's plan with deep-felt meaning it designed.
Fruitful alike in counsel and in deed,
This have we felt, this tasted, in our need.

" And many a soul that strove with him in fight,
And his great merit grudged to recognize,
Now feels the impress of his wondrous might,
And in his magic fetters gladly lies.
E'en to the highest has he winged his flight,
In close communion held with all we prize.
Extol him then! What mortals while they live
But half receive, posterity shall give."

PROCEEDINGS

OF THE

WASHINGTON MEETING, 1854.

COMMUNICATIONS.

A. MATHEMATICS AND PHYSICS.

I. ASTRONOMY.

1. RESULTS OF SOME INVESTIGATIONS RESPECTING BIELA'S DOUBLE COMET. By PROFESSOR J. S. HUBBARD, of Washington, D. C.

AMONG the many subjects of investigation offered by the astronomy of the present day, the Biela Comet merits special consideration, by reason of the accumulated material now furnished by it for comparison with our received theories. The opportunity of combining together observations of a comet at different returns to the perihelion, embracing a long interval of years, is one of the rarest, and, as shown by Encke's most recent calculations relative to the comet which bears his name, promises richer fruits than have ever been anticipated. Additional traces of other forces than gravitation have here presented themselves with renewed claims for investigation, and even the adopted theory of a resisting medium fails to explain entirely the observed discrepancies. Biela's comet has been followed with at least equal accuracy, and through a longer interval of time, and a rich harvest awaits him who will labor in this field.

But, besides all this, we have witnessed, at the recent returns to the perihelion in 1846 and 1852, the most remarkable physical phenomenon ever presented by any, even of these most remarkable bodies; and new and independent questions arise, the importance of which I will endeavor to illustrate by recounting some of the circumstances attending the phenomenon.

When first seen, in November, 1845, the comet presented no unusual appearance. It was visible for only five days, and then vanished in moonlight and clouds until the middle of December, and when refound still appeared unchanged. But on the 29th of that month, "when first observed through the five-inch refractor at Yale College, the comet was seen attended by a faint nebulous spot preceding, estimated to be rather more than a minute of space distant from its brightest point."* No publication of this fact was made at the time, and the first communication of it, of which I am aware, was in a letter to myself, dated January 15, 1846. But before this the same phenomenon had been seen, and announced publicly, by Lieutenant Maury, who also observed, not only first but oftenest, the relative positions of the two bodies.

Every one at all conversant with the history of this comet remembers the remarkable changes of apparent brightness witnessed in 1846, — how the secondary nucleus, at first very faint, suddenly equalled the best brilliancy of the primary, while the latter diminished so much as scarcely to admit of accurate observation; and how, after only a few days of pre-eminence, the usurper's glory departed, and the former order of things was restored.

Owing to the very unfavorable position of the comet with respect to the sun and earth in 1852, the observations were unusually difficult, and but four astronomers succeeded in obtaining determinations of its place. Enough was seen, however, to show that the changes of relative brilliancy of the two comets were even more striking than in 1846, and this notwithstanding their increased distance from each other. On the 25th of August, Secchi found one of the nuclei, which we will call A, and observed it until September 1. Challis observed the same on the 8th. On the 15th, Secchi found what he called the other nucleus, and described it as too faint for observation, but he determined the place of what he called the first and oldest comet. The former, too faint to be observed, was our nucleus A, which he had found twenty days before; the latter, brighter one, which we will call B, he now saw for the first time. The next day Challis saw only A; the day after, Encke saw only B; the 18th, Otto Struve observed only B; the 19th, Secchi saw both, and A was the brightest; and such alternations continued so long as the comets remained within reach. These changes were doubtless rendered more striking by the extreme faintness of both comets, hovering as they were on the limits of visibility, where a gain or loss of light that would have been overlooked in 1846 was here a question of existence to our eyes.

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* American Journal of Science, N. S. Vol. I. p. 293.

There is another remarkable circumstance anterior to all those above mentioned, but appearing only as a result of subsequent investigation, and requiring for its due appreciation a familiarity with the rapidity of change to which these bodies have shown themselves addicted. I have already said, that when first seen in 1845, and for a few days following, the comet presented no unusual appearance. But on subjecting the observations then made to the test of comparison, it was found that the places given for December 1 and 3 differed very materially from those given for November 29 and December 2. Professor Challis, who made the former, kindly communicated to me the original notes from his observing-book; but these, as well as an examination of the comparison-stars, failed to furnish any explanation of the discrepancy. To one impressed with the preconceived notion that the separation of the comet had taken place subsequently to this time, and entirely unacquainted with the exceeding fickleness of both bodies, it was difficult to believe what it has since become equally difficult to doubt; namely, that these observations of December 1 and 3 did actually refer to the *secondary comet already separated from the primary*. After the experience of 1852, the idea received more serious attention, and I then found the case to stand thus: if the body observed were the primary nucleus, the residuals in AR. and Decl. were respectively 52″ and 24″; if it were the secondary, they were 4″ and 36″, there being in both cases a large residual in Decl. of which only the difference, 12″, is chargeable upon the present hypothesis. These numbers have been modified but very slightly by subsequent reductions. Similar evidence was furnished by an observation of the comet's passage over a very small star, whose place was afterwards determined by Professor Challis, so that no doubt can now remain, that the disruption of the comet had already taken place previous to its first appearance in November, 1845.

The orbits of the two nuclei were computed in 1846, by Professor Plantamour, who based the calculation upon his own observations at Geneva. Adopting his results as correct, Santini computed with them the perturbations from 1846 to 1852, in continuation of the valuable twenty years' series which he had already given. When the comet was found in 1852, however, it was distant from its predicted place by five degrees, an enormous discrepancy, which naturally gave rise to the suspicion that unknown causes were here vigorously at work. Having already commenced some investigations respecting this comet, it was not difficult for me to examine the grounds upon which the prediction had been based, and I soon found an error in Plantamour's work sufficient to account for all the discrepancy, at least until a new determination of the perturbations shall have been obtained with the corrected elements.

Pursuing the study of the observed motions of the two comets, we come next to the important question, whether these masses, for masses they must be, thus brought nearer together than any former experience has shown, do exercise upon each other an influence sensible to us, or whether the lower limit, the limit of mass which the comets do *not* have, is to be carried lower still.

In Nos. 56 – 60 of the Astronomical Journal, I have given the results of a discussion of all the observations in 1846, undertaken with a view of determining elements which may serve as a groundwork for future investigations. I there found an orbit which represented the observed places of the primary nucleus without exhibiting any trace of the action of strange forces. New material has since been added by the receipt, first, of the observations in 1852, and secondly, of those made at Pulkowa in 1846. The latter enabled me to form a new and better system of normals for that year; the former promised a more accurate determination of the mean motions of both comets than the nature of the case would admit of from the observations in 1846 alone.

But here an unexpected difficulty presented itself, which I have not yet been able to overcome. It has been impossible for me yet to decide whether what was known as the primary nucleus in 1846 is the same with the comet A or B in 1852. The few observations obtained in this year, and the shortness of the time within which they are included, forbid the hope of deciding from them alone, and the observations in 1846 are equally well represented in either case. The only evidence furnished by the physical phenomena is the fact that the comet A was first found and oftenest observed; but from the study of the motions of the two, we shall find equally good reason in favor of the comet B, and I have therefore judged it safest to retain both hypotheses until more decisive evidence shall be furnished, which may not be until the next return to the perihelion in 1859.

Fearing lest the erroneous elements assumed by Santini for the calculation of perturbations might have sensibly affected the resulting values, I recomputed the variations of mean daily motion and mean anomaly, and, finding that Santini's values were more nearly correct than had been imagined, I adopted with the less hesitation his results for the remaining elements, as, after all, upon the *relative* motions of the two nuclei their effect would be of the second order.

A careful revision of all the observations gave now, for each nucleus, seven normals in 1846, and the observations in 1852, thanks mainly to the accuracy and skill of the Pulkowa astronomer, gave one normal of equal excellence with the others. Recomputing the theoretical places for the dates of these normals, I obtained for each hypothesis and for each nucleus a series of residuals which gave finally the corrections to be applied to the differences of the assumed elements. The results now furnished for the first hypothesis were most satisfactory, the difference of observed places in 1852 being perfectly represented, even to the tenth of a second; the sum of the sixteen residuals is two tenths of a second, and the sum of their squares is 11.5″, and no permanence of sign remaining. Neither is any trace of period shown in the results for the second hypothesis, but the residuals themselves are not so small. For the places in 1852, we have now 1.8″ and 3.3″; the sum of all the errors is 3″, and of their squares 25″, showing, as has been already said, that the evidence here is in favor of the first hypothesis.

Considering attentively the nature of the case, and the remaining uncertainties of the data, we may now safely assert, that, notwithstanding the great proximity of the two comets to each other, the apparent connection manifested by their remarkable and rapid reciprocations of brilliancy, and the traces of actual material communication visible on several occasions, we have not yet been able by the most careful study of their motions to detect any proof whatever of mutual action, and we find ourselves dealing with masses so minute as to elude all our powers of detection, and I had almost said of imagination also.

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2. On the Satellites of Uranus. By Elias Loomis, Professor of Mathematics and Natural Philosophy in the University of the City of New York.

In all the elementary works on Astronomy, it is represented, upon the authority of Sir William Herschel, that Uranus has six satellites. Herschel did certainly discover two satellites of Uranus, and determined their periods of revolution with great accuracy. He also conjectured that he had discovered four additional satellites, and he even went so far as to compute their probable periods of revolution. These periods were, however, deduced solely from the observed distances of the satellite from its primary, and not by comparing the interval between two different observations. Indeed, he was not sure of having seen either of these four satellites twice in any one year, nor did he venture to conjecture how many revolutions either of them had made between two supposed observations. The periods, therefore, assigned to these satellites rested upon a very uncertain basis, and in his last paper on this subject, communicated to the Royal Society in 1815, he says, "That there are additional satellites besides the two principal large ones, I can have no doubt, but to determine their number and situation will probably require an increase of illuminating power in our telescopes."

In the year 1838, Dr. Lamont of Munich announced that, besides the two brighter satellites of Uranus, he believed that in one single instance he had seen an additional satellite, which he considered to be the most remote of the six enumerated by Herschel. With the exception of this solitary observation of Dr. Lamont, the only evidence we have had (until recently) of the existence of more than two satellites of Uranus was derived from Herschel's observations; and he would not pronounce a decided opinion as to their number or their periods of revolution.

Our knowledge on this subject has been very much increased by the recent observations of Mr. Lassell of Liverpool. In the summer of 1840, Mr. Lassell erected a small observatory at Starfield, near Liverpool, and furnished it with a Newtonian reflector of his own construction, having an aperture of nine inches and a focal length of nine feet. In 1846, he constructed a similar telescope of much larger dimensions, having an aperture of twenty-four inches and a focal length of twenty feet. Instead of the usual small plane speculum, he employed a rectangular glass prism; and he has expressed the opinion that his telescope in this form is as efficient as one of three feet with the ordinary plane speculum. Mr. Lassell's most important discoveries have been made with this telescope. In the autumn of 1847 he obtained unequivocal evidence of the existence of a third satellite of Uranus. The orbit of this satellite was smaller than that of either of the two bright ones, and had a period of a little more than two days. On one evening, he saw what he presumed to be a fourth satellite.

In the autumn of 1851, Mr. Lassell was still more fortunate. On ten different nights in the months of October, November, and December, he saw simultaneously four satellites, and recorded their positions. The intervals were so short as to enable him to identify each satellite without danger of mistake. These satellites were the two brighter ones discovered by Herschel, and two interior ones whose periods are about two and four days respectively.

In the autumn of 1852, Mr. Lassell transported his telescope to the island of Malta, in the Mediterranean, for the purpose of securing the advantage of a lower latitude and a purer sky, and here he succeeded in obtaining a very complete series of observations of the satellites of Uranus. The nearest satellite was observed on 24 different nights; the second satellite on 23 nights; and the two brighter satellites on 26 nights. By combining these observations with those of 1847 and 1851, we are able to assign the dimensions of their orbits and their periods of revolution with great precision. The period of the nearest satellite is 2.520345 days, and the major axis of its apparent orbit in November, 1852, was 27.6″, which reduced to the mean distance of the primary becomes 27.08″.

The period of the second satellite is 4.14397 days, and the major axis of its apparent orbit in November, 1852, was 39.3″, which reduced to the mean distance of the primary becomes 38.56″.

The accompanying diagram represents the apparent orbits of the four satellites for November, 1852. The major axes of all the orbits are nearly perpendicular to the ecliptic, and the apparent exception to this rule in the case of the second satellite is probably caused by the imperfection of the observations, for the observations of 1851 did not exhibit the same anomaly. The minor axis of each of the apparent orbits is about three quarters of the major axis. The ascending node of these orbits, according to Herschel's observations, is in longitude 168°. The heliocentric longitude of Uranus, in November, 1852, was 36° 50′, making its distance from the node 131° 10′. The sine of this angle is about three quarters of radius, showing that the true orbits of the satellites differ but little from circles.

At the close of the series of observations at Malta, Mr. Lassell remarks: "On several occasions I carefully scrutinized the neighborhood of the planet to the distance of five minutes from its centre, for the discovery of other satellites. In the course of this scrutiny I made many measurements and diagrams of the positions of small points of light, which all turned out to be stars; and I cannot now resist the conviction, amounting indeed in my own mind to certainty, that Uranus has no other satellites visible with my eye and optical means."

The following table presents a summary of the results deduced from Lassell's observations of 1852. Column first shows the number of the satellite reckoned in order from the primary; column second shows the major axis of the apparent orbit, deduced solely from the observed distances; column third shows the minor axis of the apparent orbit deduced in the same manner; column fourth shows the periods of revolution derived from comparing the observations of 1847 with those of 1852; column fifth shows the mean distance of each satellite from the primary, reduced to the mean distance of the earth from the primary. These distances require a small correction to make them agree with the periods of revolution according to Kepler's law. Column sixth shows the angle of position of the apparent major axis, reckoned from the north point by the east. The mean angle of position (omitting the orbit of the second satellite, which exhibits an anomaly probably due to the errors of the observations) is 171 degrees. The angle of position measured from the pole of the equator in 1852 differs ten degrees from the angle measured from the pole of the ecliptic, making the angle of position measured from the pole of the ecliptic 181 degrees. That is, the apparent major axes of the orbits are almost exactly perpendicular to the ecliptic.

TABLE OF RESULTS.

Satellites.	Major axis of apparent orbit.	Minor axis of apparent orbit.	Period of revolution.	Mean distance from primary.	Angle of position of apparent major axis.
	″	″	d.	″	°
1	27.6	20.0	2.52035	13.54	170
2	39.3	30.4	4.14397	19.28	195
3	64.1	49.6	8.705886	31.44	173
4	87.4	68.0	13.463263	42.87	170

The planet Uranus is fast approaching the position most favorable for the observation of its satellites. It now has a North Declination of 15 degrees, which will continue to increase until 1868; and in 1863 the orbits of the satellites will appear sensibly circular. It is evident, then, that the period is near at hand when this planet can be observed more advantageously than at any former time since the first observations of Sir William Herschel, and it is presumed that astronomers will not fail to improve this opportunity to obtain all the information within the reach of our instruments, both respecting the physical peculiarities of the primary, and the number and period of its satellites.

3. A Constructive Method of Projecting Solar Eclipses.* By Chauncey Wright, Cambridge.

Problem. — Having the common tabulated data of solar eclipses, it is required to project by stereography *on the plane of reference* the outline of the shadow on the surface of the earth at any time.

These data give the position in the earth of the *plane of reference*, the position on this plane of the right cone of the shadow, the radius of the base, and the angle of the cone.

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* Lieutenant Davis presented the following paper, and at the same time remarked, that in the tables and chart of the Annular Eclipse of May 26th, published by the Smithsonian Institution and Nautical Almanac, the places of the sun were taken from Carlini's *Tables*, those of the moon from Peirce's *Tables of the Moon*, and the computations were made and the chart constructed by Mr. Kerr and Mr. Wright of the Nautical Almanac Office.

The variations of the dimensions of the cone of the shadow, and of the direction of its path on the *plane of reference*, and of the inclination of this plane to the plane of the equator, during an eclipse, may all be neglected without sensibly affecting the accuracy of a chart, even of the largest size.

Suppose planes to be drawn at equal intervals above and parallel to the *plane of reference.*

The paths of the shadow on all these planes, as on the plane of reference, may be considered as straight lines, and their projections on the plane of reference towards a point on the surface of the earth opposite the vanishing-point of the axis of the shadow or point F are given by the following formulas.

Having ρ, the radius vector of the centre of the shadow at any time, and z, the height of the plane above the plane of reference, suppose the earth a sphere with the mean radius, or that of latitude 45°, and let this radius be R; then the corresponding radius vector of the projected path will be by simple proportion $\frac{R}{R+z} \cdot \rho$.

Compute for two values of ρ, one near the beginning and the other near the end of the eclipse, the values of $\frac{R}{R+z} \cdot \rho$ for all the assumed planes or values of z.

We have then two series of points in two straight lines from the centre on the plane of reference, and joining each pair that belong to the same elevation by a straight line, we form a series of parallel lines which are the paths of the centre of the shadow on the several planes projected on the plane of reference.

The projections of the intersections of these planes with the surface of the earth are circles, of which the centres are at the centre of the plane of reference and the radii are the values of $R\sqrt{\frac{R-z}{R+z}}$.

Having ρ', the radius of the shadow (which may be taken as constant during the eclipse), and f, the angle of the cone (which does not sensibly vary), the radius of the shadow at any height, z, will be $\rho' - z \tan f$, and the base of the shadow for any plane projected on the plane of reference has the radius $\frac{R}{R+z}(\rho' - z \tan f)$, the values of which may be computed for the assumed values of z.

Draw the radii vectores of the centre of the shadow on the plane of reference for equal intervals of time during the eclipse, and the intersections of these radii with the parallel projected paths will give a series of centres for each plane of elevation, from which with the radius of the elevation, $\frac{R}{R+z}(\rho' - z \tan f)$, we can find the points of intersection of the shadows with the circle of the elevation on the earth.

Hence we have a series of points in the outline of the shadow on the surface of the earth in stereographic projection for the several intervals of time during the eclipse.

The circles of latitude and meridians may be projected by the following formulas.

Let ϕ be the latitude of the circle, and take d, the declination of the point F or elevation of the north pole from the plane of reference, as constant at its mean value during the eclipse, then the projection of the circle of latitude will be a circle having its centre in the *line of reference* at a distance from the centre of the plane $= \frac{R \cos d}{\sin d + \sin \phi}$ and with a radius $= \frac{R \cos \phi}{\sin d + \sin \phi}$.

The *line of reference* is the projection of the meridian of the point F, and the projection of any meridian making an angle λ with this meridian is a circle having its centre on a line parallel to the axis of x, and at the distance from this axis — R tan d, and from the two projected poles of the earth, R sec d.

The radius of this projected meridian is $\frac{R \sec d}{\sin \lambda}$, and its circumference passes through the poles, and also through a point in the line of the centres of the meridians, at the distance from the *line of reference*, sec. $d \tan \frac{1}{2} \lambda$.

In the chart thus constructed the circles of latitude are those of the earth, but the meridians are to be referred to the sun or the point F; therefore, in reading off the positions of the outline of the shadow, the longitudes must be reduced to the prime meridian on the earth by the value μ for the time as given in the Tables of Data.

To take account of the compression of the earth in reading off latitudes, the chart may be constructed with reduced geographical latitudes.

This method was invented for the construction of the chart of the coming annular eclipse of May 26, which, in the short time allowed for its construction, could not have been prepared by the common methods of computation. The outlines of the eclipse in the chart presented to this Association were transferred from such a chart.

Analytical solutions of geometrical problems, though generally less simple than geometrical constructions, yet furnish more accurate means for the determinations of value, — means of great importance in practical astronomy; therefore the attention of mathematicians has in modern times been especially directed to this department of their science.

The inconvenient size of geometrical constructions which are capable of representing, at the same time, quantities of very different magnitudes, renders them useless in many problems, but there is a class of problems in which successive approximations are necessary. Of these there are doubtless many cases in which the first approximations can be advantageously made by geometrical constructions or by mechanical means. To these the attention of mathematicians is called, that, by judicious combinations of geometrical constructions with trigonometrical computations, the problems of practical astronomy may be simplified.

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4. ABSTRACT OF A PAPER ON MOON AND STAR CULMINATIONS. By Professor GEORGE W. COAKLEY, of St. James College, Maryland.

THE meridian of the ephemeris being usually called the prime meridian, let us call the more eastern of the two whose difference of longitude is sought the *first meridian;* and the more western, the *second meridian.*

Let A = the right ascension of the star observed at both meridians;
a_1 = the right ascension of the moon's centre when on the *first meridian;*
δ_1 = the corresponding declination of the moon;
β_1 = the right ascension of the moon's bright limb when on the *first meridian;*
s_1 = its corresponding semidiameter.

m_1 = the moon's hourly (mean time) motion in right ascension, on the *first meridian*, expressed in seconds of sidereal time.

For the *second meridian*, let the numbers below the letters be changed from 1 to 2.

Let τ_1 = the sidereal time of transit of the moon's semidiameter over the first meridian, and τ_2 = the sidereal time of transit over the second meridian. Then we shall have, as is well known, the equations

$$\left.\begin{aligned} a_1 &= \beta_1 \pm \tau_1 \\ a_2 &= \beta_2 \pm \tau_2 \end{aligned}\right\} \text{Upper sign when the bright limb precedes.} \qquad \text{(I.)}$$

The values of τ_1 and τ_2 will be expressed by the following equations:

$$\left.\begin{aligned} \tau_1 &= \frac{\frac{1}{15}\, s_1 \,.\, \sec \delta_1}{1 - m_1\,[6.442570]} \\ \tau_2 &= \frac{\frac{1}{15}\, s_2 \,.\, \sec \delta_2}{1 - m_2\,[6.442570]} \end{aligned}\right\} \qquad \text{(II.)}$$

Let the sidereal time of the star's being on the meridian be always subtracted from that of the moon's bright limb, and let the intervals at the two meridians be denoted by i_1 and i_2, so that we shall always have

$$\left.\begin{aligned} i_1 &= \beta_1 - \text{A} \\ i_2 &= \beta_2 - \text{A} \end{aligned}\right\} \qquad \text{(III.)}$$

and hence

$$\left.\begin{aligned} a_1 &= \text{A} + i_1 \pm \tau_1 \\ a_2 &= \text{A} + i_2 \pm \tau_2 \end{aligned}\right\} \qquad \text{(IV.)}$$

Hence $a_2 - a_1 = i_2 - i_1 \pm (\tau_2 - \tau_1)$, which is independent of the star's place, provided the same right ascension be attributed to the star at both meridians.

Let now τ_1 = the mean time, counted from the prime meridian, at which the moon's centre has the right ascension a_1, and τ_2 = the mean time corresponding to the right ascension a_2. The values of τ_1 and τ_2, to the nearest second of sidereal time, may readily be taken from the list of Moon-Culminating Stars in the Nautical Almanac, almost or quite by simple inspection; so that a_1 and a_2 are easily found to this degree of accuracy. This is sufficient to give approximate values of τ_1 and τ_2 to the nearest minute of mean time, by a very simple computation based upon the hourly right ascensions of the moon. Let these approximate times be denoted by τ_1' and τ_2'. With these times find by interpolation with second differences, from the moon's hourly positions, the corresponding accurate values of the right ascensions a_1', a_2', the declinations δ_1, δ_2, and the accurate hourly increments of right ascension, m_1, m_2; find also the semidiameters, s_1, s_2. With these data compute accurately the values of τ_1 and τ_2 by formulas (II.). Then a_1 and a_2 will be known from (IV.) with great accuracy.

Let now $\tau_1 = \tau_1' + t_1$, and $\tau_2 = \tau_2' + t_2$, t_1 and t_2 denoting the number of *seconds of mean time*, by which τ_1' and τ_2' must be corrected, in order to give τ_1 and τ_2.

We shall then have the following equations: —

$$\left.\begin{aligned} a_1 &= a_1' + \frac{m_1}{3600} \cdot t_1 \\ a_2 &= a_2' + \frac{m_2}{6300} \cdot t_2 \end{aligned}\right\}$$

whence

$$\left.\begin{aligned} t_1 &= \frac{a_1 - a_1'}{m_1\,[6.443697]} \\ t_2 &= \frac{a_2 - a_2'}{m_2\,[6.443697]} \end{aligned}\right\} \qquad \text{(V.)}$$

Hence the values of τ_1 and τ_2 become known.

Having obtained these mean times corresponding to the moon's right ascension a_1 and a_2, at the instants when its centre was on each of the meridians, we shall know $\tau_2 - \tau_1$, the *mean time* which the moon's centre employed in passing from the *first* to the *second* meridian; hence we shall also know the sidereal equivalent of $\tau_2 - \tau_1$, which we shall denote by $\{\tau_2 - \tau_1\}$. Let $\lambda =$ the arc of the equator between the meridians, expressed in sidereal time. Then if a star had been on the first meridian at the same time with the moon's centre, this star after the lapse of the sidereal time $\{\tau_2 - \tau_1\}$, which brings the moon's centre to the second meridian, would be west of the second meridian by the amount of the moon's change of right ascension, namely $a_2 - a_1$; and the star would consequently have described the arc of sidereal time $\lambda + a_2 - a_1$, which must therefore be equal to the sidereal time $\{\tau_2 - \tau_1\}$ that has elapsed since the star was on the *first* meridian. Hence

$$\{\tau_2 - \tau_1\} = \lambda + (a_2 - a_1);$$

and hence

$$\lambda = \{\tau_2 - \tau_1\} - (a_2 - a_1). \qquad \text{(VI.)}$$

The remainder of the paper is taken up with a discussion of the theoretical and practical errors to which the proposed method might be liable, and with its application to three different examples. The first example is to determine the difference of longitude between Greenwich Observatory and that of Georgetown College, from observations made by Professor Curley together with others furnished by Professor Airy. From the transits of the moon's first limb and of Aldebaran on the 4th of February, 1846, I find by the proposed method $\lambda = 5^{h.}\ 8^{m.}\ 18^{s}.84$.

Professor Curley states two results for this example, namely $5^{h.}\ 8^{m.}\ 19^{s}.48$ and $5^{h.}\ 8^{m.}\ 18^{s}.93$; giving a difference between the two methods, in one case of $0^{s}.64$, and in the other of $0^{s}.09$.

The second example is to determine the difference of longitude of the same observatories by means of the transit on the 10th of March, 1846. By the method here proposed I find in this case $\lambda = 5^{h.}\ 8^{m.}\ 17^{s}.29$. Professor Curley finds $\lambda = 5^{h.}\ 8^{m.}\ 17^{s}.80$, giving a difference of $0^{s}.51$.

The third example is that given by Dr. Brünnow in his "Lehrbuch der Sphärischen Astronomie," page 354. I find in this case for the difference of longitude between the observatories of Bilk and Hamburg, $\lambda = 12^{m.}\ 52^{s}.81$. Dr. Brünnow finds $\lambda = 12^{m.}\ 52^{s}.83$, giving a difference of $0^{s}.02$.

5. On the Results of the Chronometric Expeditions of the Coast Survey for determining the Longitude of Cambridge, made in 1849, 1850, and 1851.

By G. P. Bond, of the Observatory of Harvard College.

The Superintendent of the Coast Survey authorized, in the years 1849, 1850, and 1851, two expeditions for the transportation of chronometers between the observatories of Cambridge, Mass., and Liverpool, for determining a standard of longitude for points upon this continent.

The paper communicated explained the manner of conducting the voyages, the organization and general superintendence of which was intrusted to Mr. W. C. Bond, Director of the Observatory of Harvard College, and the methods of computation adopted for deducing the final results.

The total number of voyages was nineteen, with ninety-three different chronometers.

In conducting the calculations, every means has been employed to insure accuracy. The combinations have been effected throughout by processes based upon the theory of probabilities.

The result of the expeditions gives for the longitude of Cambridge, West of Greenwich,

$$4^{h.}\ 44^{m.}\ 30^{s.}.66;$$

with a probable error of two tenths of a second.

This result continues a chain of chronometric longitudes from Pulkova in Russia to the eastern coast of America.

6. ON THE DETERMINATION OF THE LONGITUDE OF FRONTERA, EL PASO, AND SAN ELCIARIO, RESULTING IN THE DETERMINATION OF THE LONGITUDE OF A CARDINAL POINT IN THE SURVEY OF THE BOUNDARY BETWEEN THE UNITED STATES AND MEXICO. By MAJOR W. H. EMORY, of Washington.

I HAVE on a former occasion, though not in person, presented this Association with a general view of the plan proposed for conducting the survey of the boundary between the United States and Mexico. In the execution of that plan, the determination of the longitude of three cardinal points was decided on, by the most certain methods known of which circumstances would admit; one on the Pacific, one on the Atlantic side, and one near the middle of the continent.

That near the middle of the continent, from being the most inaccessible and the least known, may claim the first interest, and I now give a short account of it, with some suggestions in regard to improvements in existing lunar tables.

The observations at two stations, Frontera and San Elciario, were combined to arrive at the results here given; both of these stations are on the Rio Grande, called by the Mexicans Rio Bravo.

The longitude of Frontera, from forty-two moon culminations, extending through four lunations, computed from the predicted place of the moon and moon-cul-

	h.	m.	s.
minating stars given in the Greenwich Ephemeris for 1851 – 52,	7	5	55.30
The same, after applying the correction due for corresponding observations made at Greenwich, and furnished in manuscript from the observatory at that place, is : —			
For the 1st lunation, December and January, 1851 – 52,	7	6	11.38
" 2d " January and February, 1852,	7	6	13.34
" 3d " February and March, "	7	6	14.73
" 4th " March and April, "	7	6	12.48
Mean,	7	6	12.98
Difference due to correction,			+ 17.68
The longitude of San Elciario, deduced from observations on forty moon culminations, computed from the predicted place of the moon and moon-culminating stars given in the Greenwich Ephemeris for 1852,	7	4	46.55
The same, after applying the correction due for corresponding observations made at Greenwich, is : —			
For the 1st lunation, January and February, 1852,	7	5	2.52
" 2d " February and March, "	7	5	4.31
" 3d " April, "	7	5	3.44
Mean,	7	5	3.42
By combining these observations with corresponding ones at Cambridge, we get	7	5	4.30
Difference due to correction,			+ 17.75

The coincidence between these differences is satisfactory. The computations were made by independent computers; the first by Assistant Clark, the second by Assistant Donoghue. The observations for the first were made by myself, those at San Elciario by Lieutenant Smith.

To give greater effect to the results, and to establish, beyond the probability of future change, the longitude of Frontera, which is about the middle longitude of the continent, and is a cardinal point in the survey of the boundary between the United States and Mexico, an attempt has been made to connect the observatory of San Elciario with that of Frontera by flashes of gunpowder, and the following is the result. It should be observed that the Frontera here mentioned is not the Frontera of the old maps of North America. Frontera signifies a a frontier town, and is the name given by its proprietor to a newly constructed hut, built immediately on the ratification of the Treaty of Guadalupe Hidalgo, at a point some eight miles above El Paso, where it was supposed the boundary under that treaty would leave the Rio Grande. The difference of longitude between Frontera and San Elciario, observed by flashes on the nights of the 14th, 18th, and 19th February, and 14th March, 1852, was found to be $1^{m.}\ 7^{s.}.3$. The difference of longitude by observations on the moon and moon-culminating stars is $1^{m.}\ 8^{s.}.58$, which agrees with that determined by flashes within $1^{s.}.3$. Now, assuming the difference determined by flashes as correct, and giving equal weight to the observations at each station, the longitude of Frontera will be diminished by $0^{s.}.6$, and that of San Elciario will be increased by the same quantity; so that the final result will be,—

	h.	m.	s.
Frontera west of Greenwich, . . .	7	6	12.3
San Elciario, ...	7	5	4.9

The longitude of the observatory at El Paso, erected by the Mexican Commission, two hundred feet south and five hundred feet west of the cathedral tower, was determined, by flashes made on the nights stated in the preceding part of this article, to be $15^{s.}.94$ east of Frontera; hence the longitude of El Paso, $7^{h.}\ 5^{m.}\ 56^{s.}.36$.

A discussion of the longitudes of the Presidio del Norte and at Eagle Pass on the Rio Grande shows a difference between the longitude obtained by using the Greenwich Ephemeris, and that obtained by using corresponding observations at Greenwich, to be in the first place $+19^{s.}$, and in the second $+17^{s.}.1$. Comparing these with the like quantities obtained at Frontera and San Elciario, made in the same year, it will be seen that there is a coincidence, showing that the error in the predicted place of the moon is very nearly uniform for the same year. The difference between the longitudes obtained in 1849, on the Pacific coast, between those computed by the Greenwich Ephemeris, and those obtained by corresponding observations at Greenwich, was pretty generally $+12^{s.}.0$. This confirms the importance of a thorough revision of the Lunar Tables. The uniformity observable in these results shows the probability of detecting the error, and eliminating or reducing it.

It is well known to many of this Association, that the Greenwich observations for any given year are not received in the printed form in this country for nearly two years after they are made, in consequence, no doubt, of the time devoted to their elaborate and thorough computation. The observations from which the above results were obtained were furnished the United States Boundary Survey in manuscript by the officers of the Royal Observatory at Greenwich, in anticipation of the published report. By this act of courtesy, I am enabled at once to complete the final results, and use them in the projection of the maps of the survey; the execution of which would otherwise be delayed one or more years.

The Observatory at Greenwich is under no obligation to us whatever, and, after stating the facts above, I am sure I shall receive the cordial concurrence of this Association, as I know I shall receive that of my government, in expressing to Professor Airy and the officers of that establishment our high appreciation of their contribution to astronomical science in general, and of the aid afforded in this particular instance to a branch of science in the United States.

I have also to express my thanks to Mr. Bond for the corresponding moon culminations observed at Cambridge, Mass., and to Lieutenant Maury and the officers of the National Observatory for observing the north polar distance of stars used by me in the determination of the latitude at different places.

7. SUGGESTIONS RELATIVE TO THE OBSERVATION OF THE SOLAR ECLIPSE OF MAY 26, 1854. By STEPHEN ALEXANDER, LL. D., Professor of Mechanical Philosophy and Astronomy in the College of New Jersey.

THE title of this paper is sufficiently indicative of its object. The phenomena to which it relates will be considered in their natural order; but, that the notice of them may not afterward be interrupted, it may be well enough to describe at the outset an arrangement of screen-glasses which may be found to be convenient when a ready adaptation, and especially a rapid change, of them is desired.

To effect the objects last mentioned, several screen-glasses of various colors might be arranged in a circular frame movable about a central pivot, so that each screen-glass might, in its turn, be brought in front of the eye-piece of the telescope, at the pleasure of the observer.

The rotary motion might be rendered more steady by the pressure of a small spring with a tooth falling into a shallow dent in the revolving frame; as is the case in the adjustment of dark glasses made use of in some sextants. It would, moreover, seem to be advisable that the succession of tints of the several screen-glasses should be such, that the eye would not be too much "blinked" or rendered insensible by a sudden change of one for another; the order being somewhat like the following: *white*, *yellow*, *orange*, *red*, *violet*, *blue*, and (if there be room for them) then *green* and *greenish-yellow;* to come back, in the circuit, to *white* again.* The white tint of sun-light is preserved when the view is through Dr. Maskelyne's screen of liquid ink, or the like effect may be obtained by a suitable combination of screens of different colors, such as, for instance, *violet* and *green glasses*, the number of violet glasses being in excess. The singularly tinted glass which goes by the name of *London smoke* is, however, perhaps the best of all for presenting a white image of the sun, as by it all colors are transmitted, although with a diminished intensity.†

Proceeding next with the phenomena to be observed, those demanding special attention will be found marked with an asterisk. In thus distinguishing some of them, the author has been governed by the unanimous opinion of the Committee of the American Association as expressed at their meeting; namely, that the *times* of the beginning, end, &c. ought to be carefully noted at places whose geographical positions are *well* determined; but that, at other places, *the observation of the physical phenomena should be carefully attended to*, even if the precise time of their occurrence should thereby fail to be noted.

Having regard, as already intimated, to the natural order of sequence of the phenomena, we have, —

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* A smoked glass ought also to have a place in this series.

† My attention was called to this highly suitable material by Professor John F. Frazer, in the course of the deliberations of the committee on this eclipse. It has itself some variety of tint, but as many screens of it as may be requisite may be combined; the aperture of the object-glass being moreover reduced if necessary.

(*A.*) *Phenomena observable before the Beginning of the Eclipse.*

(1.)* The idea suggested by M. Delisle, that the border of the moon might be seen when off the sun, seems to have derived additional confirmation from the observations made in Europe during the total eclipse of 1851.

The moon seems to have been in part visible under such circumstances, or rather her outline to have been seen, because *the luminous substance surrounding the sun* was then partially hidden.

Now, as white is the complementary color to black, it would seem that the approach of the moon before the first contact might be best discerned, if visible at all, through a screen showing a *white* sun; i. e. one of those already described, and especially that last mentioned. It is desirable that the white tint under such circumstances should be very perfect, as otherwise the complementary and accidental color would seemingly overspread the region beyond the sun's limb. If, however, any such screen should prove after all to be unserviceable in showing where the field is obscured by the interposition of the moon, a screen of any of the other colors, secured in the movable frame, might at once be introduced, and the position of the moon's limb perhaps be recognized, through the colored glass thus found to be best adapted to the purpose.

(*B.*) *At the Beginning of the Eclipse.*

(2.)* Prominent points indenting the sun's disc in advance of the moon's general outline. These might very probably be visible through a screen-glass of some one special color (*red* perhaps), and not discernible through another of a different tint. The difference, if any, would require the notice of *two* or *more* observers, severally furnished with different-colored screen-glasses.

(3.)* "Dark" lines or "indentations," analogous to Baily's (?). (Professor W. R. Johnson saw them at Philadelphia, in 1838, for eight seconds.)

It would be better, also, to have more observers than one to note these, the observers being severally furnished with screen-glasses as for the observation of phenomenon (2).

(3′.) The time of the unequivocal contact of the limbs.

(*C.*) *As the Eclipse progresses.*

(4.) The bending or distortion of the cusps of the uneclipsed portion of the sun, as the moon advances.

(5.) An agitation of the edge of the moon's disc. (Is reported by some observers; but may have been due to variable refraction of those observers' atmosphere.)

(6.) Corruscations across the moon's disc.

(7.)* An illuminated band bordering the moon's disc, and distinguished from the rest of the uneclipsed portion of the sun by its seemingly greater intensity.

(8.)* Special phenomena accompanying the eclipse of a solar spot. (Rev. Mr. Dawes of Ormskirk has recorded and represented a remarkable instance of the illumination of a solar spot, and Professor L. R. Gibbes of Charleston, S. C. observed an apparent repulsion of a solar spot in 1846.)

(8′.)* The polarization of light under these circumstances.

(9.) Luminous projections on the moon's disc.

(10.)* (Analogous to (1).) Projection of the moon's disc beyond that of the sun, in this more advanced state of the eclipse.

(11.) Color of the moon's disc. This may be found not to be independent of the color of the screen-glass. The latter should therefore be specified (in the account of any observation), for that as well as many other reasons.

(*D.*) *Immediately previous to the Formation of the Ring.*

An appearance like a twilight between the cusps. With regard to this should be noticed: —

(*a.*)* The time of its first appearance.

(*b.*)* Its extent, in the direction of both its polar coördinates; viz. along the circumference of the moon's disc, and in the direction of the moon's radius outward.

(*c.*)* The color of the light, if invariable; or

(*c′.*)* Any changes in its intensity or its color.

(*d.*)* Its apparent motion, if any, along the edge of the moon's disc.

(*e.*)* The polarization of this light, if discoverable.

(*f.*)* Whether the light be found to undergo any special change just as the ring is about to form.

(*g*.)* The actual intensity of the light; which may be determined in some measure by the depth of tint of the screen-glass through which it is discernible: as was done by Bessel in 1836.†

(*E.*) *At the Formation of the Ring.*

(*a*.)* The form and changes of form of the cusps. These not unfrequently have been found to be united rapidly by a serrated bright edge, assuming sometimes (and later) the appearance of a row of beads.

(*b*.)* The apparent motion of such portions of the sun's disc along the moon's edge.

(*c*.)* If also attainable, the time of commencement and exact duration of such appearances.

(*d*.)* Their color and its changes, if any.

(*e*.)* The time of the complete formation of the ring.

(*f*.)* *The dark lines* of Baily. These should be very carefully sought for; the rather, as this eclipse may be regarded as a return of that of 1836. The several changes of those dark lines should also be noted.

(*g*.)* Whether the ring when completely formed is any broader or any narrower than the beads were.

(*F.*) *While the Ring endures.*

(*a*.) Project the shadow of a ball upon a suitable screen, and observe not only its annular form, but also whether it be found to exhibit any colored fringes, and if so, then their variety and extent.

(*b*.) Having obtained a solar spectrum, observe the character and disposition of its colors, and also *its dark lines.*

(*c*.) Any special changes of outline or beams of light indenting the moon, as she recedes from or approaches the sun's edge. (To Baily the moon's edge appeared more convex on the side on which the ring was, for the moment, the narrower.)

(*d*.) The color of the moon's disc. This may, as before intimated, not be independent of that of the screen-glass. (To Baily, as seen through a red glass in 1836, the moon appeared of a purple tint during the existence of the ring.)

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† (*e*) and (*g*) may be more readily determined at stations near to the limit of the annular eclipse, where the moon's edge will appear nearly in contact with that of the sun for some time.

(*e.*) The annular breadth of the ring, determined by a micrometer.†
(*f.*) The moon's diameter.†

(*G.*) *At the Rupture of the Ring.*

The same phenomena as at the formation; though in the inverse order.

(*H.*) *Immediately after the Rupture of the Ring.*

As at the corresponding period before the formation; but also in the inverse order.

(*I.*) *Between the Rupture of the Ring and the End of the Eclipse.*

As in the analogous period after the beginning.

(*K.*) *At the End of the Eclipse.*

Time of the last unequivocal contact.

(*L.*) *Continued Adhesions, &c.*

As at the analogous period preceding the first decided contact.

(*M.*) *After the End of the Eclipse.*

Same appearance, (1), to be sought for as before the beginning.

Miscellaneous Observations at Various Times during the Eclipse.

1.* Temperature of the atmosphere in the sun and in the shade.
2. Photometric observations (?).
3. Change in the dew-point and deposition of dew.
4. Polarization of the light of the atmosphere.
5. Polarization of the heat of the same.
6. The peculiar tint of the sky.
7. The course, as well as variable force, of the wind.
8. Change, if any, of magnetic intensity (?).

When the number of phenomena, all to be noted in a very short time, was very great, only the more indispensable observations have been marked with the asterisk, though several of the others would be of interest if they could be obtained; and they might perhaps be completed, if several observers were collected at one station.

For the purpose of a more ready reference, the various particulars which have now been specified will here be concisely recapitulated.

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† With a view to determine the extent of inflection, and also that of irradiation if the sun's diameter be measured, which need not be done at that moment.

TABULAR VIEW.

(1.)* Observation of the edge of the moon's disc when off the sun.
(2.)* Indentations of prominent points
(3.)* Special roughness or dark lines — *At the Beginning.*
(3′.) First unequivocal contact
(4.)* Distortion of the cusps.
(5.) Agitation at the edge of the moon's disc (?).
(6.) Corruscations across the moon's disc.
(7.)* An illuminated band bordering the moon's disc.
(8.)* *Effects on solar spots.*
(8′.) Polarization of light at solar spots.
(9.) Luminous projections on the moon's disc.
(10.)* Analogous to (1), projection of the moon's disc beyond that of the sun.
(11.) Color of the moon's disc.

Previous to the Formation of the Ring.

The light between the cusps.

(*a.*)* The time of its first appearance.
(*b.*)* Its extent in both length and breadth.
(*c.*)* Its color, if invariable; or
(*c′.*)* Changes in the light's intensity or its color.
(*d.*)* Its apparent motion, if any, along the edge of the moon's disc.
(*e.*)* Its polarization.
(*f.*) Its special changes just before the formation of the ring.
(*g.*) Its intensity as indicated by the depth of tint of the screen-glass traversed by it.

At the Formation of the Ring.

Serrated appearances or beads.

(*a.*)* The form and changes of form of the seeming fragments.
(*b.*)* Their motion along the moon's edge.
(*c.*) Time of their commencement, and their exact duration.
(*d.*)* Their color, and its changes.

(*e.*) Time of the complete formation of the ring.

(*f.*)* *The dark lines of* Baily: same particulars to be noted with regard to them as those specified in the case of the beads.

(*g.*) Whether the ring when completely formed is found to be any broader or any narrower than the beads.

While the Ring endures.

(*a.*) The projected shadow of a ball, and its colors, if any, and their extent.

(*b.*) The extent and variety of the colors of the solar spectrum, and their dark lines.

(*c.*) The special changes of outline of the moon by encroachment of light, or by distortion of the disc.

(*d.*) The color of the moon's disc.

(*e.*) The angular breadth of the ring.

(*f.*) The moon's diameter.

At the Rupture of the Ring.
Immediately after the Rupture of the Ring.
Between the Rupture and the End of the Eclipse.
At the End of the Eclipse, and immediately thereafter; and
After the End.

As at the respectively analogous periods in the former part of the eclipse, but in an inverse order in every case.

Miscellaneous Observations at Various Times.

1. Temperature in sun and shade.
2. Photometric observations (?).
3. Change of dew-point and deposition of dew.
4. Polarization of the light of the atmosphere.
5. Polarization of its heat.
6. Tint of the sky.
7. Course and variable force of the wind.
8. Change, if any, of magnetic intensity (?).

8. ON THE RELATIVE VALUE OF THE DIFFERENT ASTRONOMICAL METHODS OF DETERMINING THE LONGITUDE. By CAPTAIN C. H DAVIS, Superintendent of the American Nautical Almanac.

THE lists of occultations of the stars and planets by the moon is about three times as great in the American Ephemeris as in any other, and the increased labor bestowed upon this part of the work has a special reference to the character of the country, and the wants of our practical astronomers and surveyors.

For this and other obvious reasons, it has become a matter of particular interest, and indeed duty, with me, to ascertain which is the most accurate of the methods in common use for determining the longitude, in order to know in what direction the labor of the Ephemeris can be best applied.

The practical availability of any astronomical phenomenon must depend upon the nicety with which it can be observed; and this is independent of any subsequent accuracy of computation. This test of observation is the one applied in the present investigation.

The simultaneous observations of occultations by the moon, of the eclipses of Jupiter's satellites, and of solar eclipses to be found in the first twenty or thirty volumes of the *Astronomische Nachrichten*, have been collected together. These observations naturally distributed themselves in the following manner.

The occultations by the moon were grouped under the four heads of immersion and emersion upon the dark and bright limbs; the eclipses of Jupiter's satellites were divided into the immersions and emersions of the different satellites; and the eclipses of the sun into the first and last contacts, the other phases of the latter phenomenon not being sufficiently numerous for discussion.

First, of the occultations.

In the case of the disappearance on the dark limb, twenty-nine series of observations were collected, the number in the different series varying from twenty-three to four. The probable error of each series was determined separately.

In the case of the reappearance on the dark limb, the number of series is twelve, which were treated in the same manner.

Of the disappearances on the bright limb, the number of series is nine.

Of the reappearances on the bright limb, the number of cases comprising instances enough to authorize a deduction diminished, as might have been expected, to two.

In this part of the discussion, the simultaneous observations were so numerous, amounting altogether to more than four hundred, that it was possible, as it was certainly most desirable, to consider the undivided observers separately; so that what is called a series passes under the name of an individual. The result, therefore, in each series, appears as the probable error of observation of the observer.

In the cases of Jupiter's satellites and the solar eclipses, all the simultaneous observations are combined into one set, without distinction of names.

The results of the investigations are as follows: —

DARK LIMB.

IMMERSION.

Observers.	No. of Obs.	Personal Equation.	Probable Error.
Preuss	16	.388	.171
Walbeck	7	.00	.039
Wrangel	5	.04	.06
Sabler	10	.42	.11
Sawitsch	4	.63	.135
Döllen	5	.43	.275
Kalmberg	5	.25	.157
Bittner	15	.31	.354
Greig	13	1.28	1.39
Dahl	23	.23	.161
Apostoli	7	.43	1.06
Clausen	7	.20	.476
Hansen	5	.09	.325
Petersen	6	.253	.157
Selander	4	.025	.173
Adie	5	.36	.563
Auger	9	.22	.369
Busch	13	.097	.231
Argelander	4	.0	.0
Hoyos	5	.08	.571
Busolt	7	.104	.385
Cuesta	6	.05	.261
Castañeda	5	.40	.523
Funk	24	.002	.107
Weyer	8	.138	.474
Manganari	5	.644	.843
Ballo	11	.257	.575
Wiedmann	4	.675	.387
29. Ryseans	7	.071	.248
	245		

EMERSION.

Observers.	No. of Obs.	Personal Equation.	Probable Error.
Apostoli	8	.001	.182
Preuss	14	.44	.916
Greig	4	.194	.202
Argelander	5	.24	.494
Riggs	4	1.17	.437
Mason	4	.138	.308
Cuesta	7	.043	.477
Castañeda	4	.05	.067
Funk	21	.213	.597
Weyer	16	.669	.566
Jürgensen	7	.4	.451
12. Fuss	4	.325	.98
	98		

BRIGHT LIMB.

IMMERSION.				EMERSION.			
Observers.	No. of Obs.	Personal Equation.	Probable Error.	Observers.	No. of Obs.	Personal Equation.	Probable Error.
Cuesta	6	4.91	2.47	Sabler	4	5.54	5.13
Riggs	6	2.27	1.62	2. Funk	6	2.43	3.01
					10		
Argelander	4	.5	2.08				
Preuss	12	1.32	2.33				
Funk	4	2.31	2.56				
Peters	6	1.62	3.11				
Fuss	5	1.28	1.36				
Landahl	5	5.8	5.08				
9. Schweitzer	5	2.88	3.61				
	53						

If each one of the above series is taken collectively, it will appear that the probable error of all observations

	s.
of an immersion on the dark limb of the Moon is	0.49
of an emersion on the dark limb	1.96
of an immersion on the bright limb	3.19
of an emersion on the bright limb	3.94

Not many simultaneous observations of eclipses of Jupiter's satellites were obtained from the journals. The results of those which were discussed, twenty-nine in all, are as follows: —

	Probable Error. s.
First Satellite, immersion	3.59
" " emersion	2.07
Second Satellite, "	3.58

In the case of the eclipses of the Sun, the result is, —

For the beginning, the number of cases being 24, ...	4.96
For the end, the number of cases being 37, . . .	5.24

Whole number of cases 61

The decided advantage of the method by occultation on the moon's dark limb will be an inducement rather to increase than diminish the lists in the American Ephemeris.

9. ABSTRACT OF A PAPER ON THE DIFFERENCE OF PERSONAL EQUATION IN TRANSIT OBSERVATIONS OF THE SUN AND OF THE FIXED STARS. By JOSEPH WINLOCK, Assistant in the Nautical Almanac Office.

IT has been suggested by several astronomers, that personal equation is not the same in observations of the sun or moon as it is in observations of the fixed stars. Dr. Gould, in a valuable paper on personal equation, read at the Cleveland meeting of this Association, called attention especially to the fact, that it is not the same for all celestial objects; but I know of no attempt to make a comparison between observations of the sun and moon and of the fixed stars, calculated to demonstrate the existence or amount of the difference of effect of personal equation upon them.

In order to settle the question of the existence of such a difference, I have examined the whole series of Greenwich transit observations of the sun, from 1836 to 1851 inclusive, taking the errors of the Nautical Almanac given by each observer separately, and reducing them to some one as a standard. The observations having been corrected previously for personal equation deduced from star observations. Any remaining difference between them, not accounted for by change of error of the tables, I have referred to the effect of a personal equation peculiar to the sun observations.

The details of the investigation are not yet ready for publication; but the observations show conclusively, that they are affected by a personal equation not taken account of in their reduction, which in one or two observers amounts to $0^{s}.17$; in the others it is less than this, and generally of about the same average value for each throughout the whole series of his observations, except in one instance, that of Mr. Henry, where there is a marked change extending with some regularity over about one year, 1836, after which it is nearly constant.

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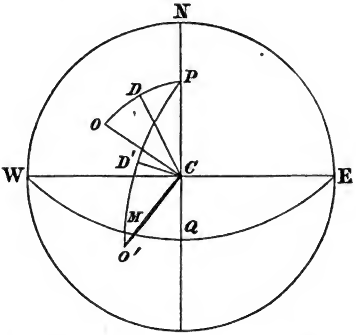
10. METHOD OF OBSERVING AT SEA FOR THE DETERMINATION OF THE LATITUDE, LONGITUDE, AND VARIATION OF THE COMPASS. By O. C. BADGER, Passed Midshipman, United States Navy.

THE importance of taking frequent observations at sea, both night and day, for determining the position of the vessel, as well as the variation of the compass, — and, by recommendation of the late maritime conference at Brussels, this is ordered to be done in all government vessels, — has induced me to suggest a plan of taking observations which will greatly reduce the labor of computation. It applies to all celestial objects, but is used with greatest advantage in observations of the stars and planets, the declination of which is constant, or the rate of change very slow. Used in connection with Sumner's method, the labor of computation will be very much lessened, and will, I think, make that method a favorite one for night observations. It consists in this: In observing when the altitude of any celestial object is equal to its declination.

DEMONSTRATION OF FORMULÆ.



The figure is a projection of the celestial hemisphere on the plane of a great circle E N W, of which the point C, the place of the observed object, is the pole. E Q W is the Equator, and P is its pole; O is the zenith of the observer, and O P, a circle of declination, is his co-latitude; C O is the distance of the object from the zenith of the observer; C P, its polar distance.

Now C O = C P, for the observation is taken when the declination and altitude of the object are equal. From the point C to the middle of the arc O P draw the arc C D; the triangle C P O being isosceles, the angle C D P is a right angle, and D P = ½ co-latitude. The angle D P C, which we will call h, is the hour-angle of the observed object.

Let D = Declination, then, by Napier's Rules, we have,

$$\cos h = \text{tang D tang } \tfrac{1}{2} \text{ co-lat.} \qquad (1.)$$

In case the observer and the object are on different sides of the Equator, or, which is the same thing, his latitude and its declination are of different names, we have,

$$\cos h = \text{tang D tang } \tfrac{1}{2}\,(90° + \text{lat.})\,; \qquad (2.)$$

for, suppose the observer at O′, then O′ M = his latitude, and P M = 90° ; therefore P D′ = ½ (90° + lat.).

In an isosceles triangle the angles opposite the equal sides are equal; therefore the angle D P C = D O C, but D O C is the azimuth of the object; therefore the hour-angle of the object is also its azimuth, and no further calculation is necessary to obtain it.

If we have the hour-angle and wish to obtain the latitude, in (1) we have,

$$\text{tang } \tfrac{1}{2} \text{ co-lat.} = \cos h \text{ cotang D}\,; \qquad (3.)$$

in (2) we have,

$$\text{tang } \tfrac{1}{2}\,(90° + \text{lat.}) = \cos h \text{ cotang D.} \qquad (4.)$$

The Selection of Objects.

When the latitude of the observer and the declination of the object are of the same name, if ½ co-latitude be greater than the polar distance, the case is impossible; and the more nearly equal these quantities, the nearer the meridian will the object be when it is to be observed. When they are of different names, if ½ (90° + lat.) be greater than the distance of the object from the nearest pole, the case is impossible; and the more nearly equal these quantities, the nearer the meridian will the object be when it is to be observed.

In selecting an object for observation, if the latitude is sought, one should be chosen the altitude of which does not become equal to the declination until it is near the meridian; but if the hour angle is sought, the observation should be made as far from the meridian as possible, for then the body is rising or falling most rapidly, and a slight error in altitude does not greatly affect the result.

In observing for the hour-angle, both in the Northern and Southern hemispheres, in general, observe those objects the declination of which is not less than 12° or greater than 60°. In high latitudes observe those that pass through the zenith of the observer, or between his zenith and the equator; in other words, the declination should not in high latitudes be greater than the latitude of the observer.

In low latitudes select those, the declination of which, being greater than 12°, is most nearly equal to the latitude of the observer.

It may be convenient to observe objects not in the same hemisphere with the observer; in this case, those should be selected passing nearest the zenith of the observer. Formula (2) applies to this case.

PREPARATIONS FOR, AND TAKING OF, AN OBSERVATION.

If the sun or moon is to be observed, a few minutes before the object reaches an altitude equal to its declination, (a practical observer can tell this by the eye, but if he be not practised, he may measure with his sextant, and, as the object approaches the sought altitude,) note with a chronometer or watch the Greenwich time, and take from the Nautical Almanac the declination, and with the hourly difference calculate as nearly as possible what the declination will be when it reaches the sought altitude. An approximation sufficiently correct for all practical purposes will be obtained. From this determined declination, subtract the semidiameter, if the lower limb be observed, if the upper, add it; subtract the parallax, and add the dip and refraction. The sextant is then to be set to this quantity, and when the object reaches this altitude, note the time by watch or chronometer, and the observation is completed.

If a planet is to be observed, take its declination from the Almanac for the corresponding day, and calculate approximately its declination for the time of observation. From this declination subtract the semidiameter, subtract the parallax, and add the dip and refraction; set the sextant to this quantity, observe, and note the time as before.

For a star, take its declination from the Almanac, and add to it the dip and refraction; set the sextant to this, observe, and note the time as before.

COMPUTATION.

When the sun is observed, to obtain the apparent time, and the sun's azimuth.

RULE. — *If the latitude and declination are of the same name, subtract the latitude from* 90°, *and divide the remainder by* 2; *if of different names, add it to* 90°, *and divide by* 2; *to the log. tangent of the quotient, add the log. tangent of the sun's declination, and the sum of these two logarithms, rejecting the tens in the index, is to be sought for in the column of cosines, and from the column marked P. M. in*

Bowditch's Navigator, take out the corresponding time, which divided by 2 is the hour-angle; if in the afternoon, it is the apparent time; if in the forenoon, subtract it from 12 hours, the remainder is the apparent time.

The azimuth is to be taken out in degrees and minutes at the top or bottom of the page, as the case may be, and is reckoned from the North when the sun is in north declination, and from the South, when in south declination.

N. B.— The azimuth in this case is always an acute angle, except when D = 0, *in that case it is equal to* 90°.

Example I.

In latitude 35° N., in the afternoon, when the sun's declination was 20° 30′ N. and its semidiameter 16′, set the sextant at 20° 20′ 24″, having allowed 4′ for dip, 8″ for parallax and 2′ 32″ for refraction, and noted when with this altitude the sun's lower limb touched the horizon. Required the apparent time and the sun's azimuth.

Dec.	20° 30′	log tang	9.57274
½ co-lat.	27° 30′	" "	9.71648
		Cos hour-angle	9.28922

Searching in the column of cosines with this log., I find the corresponding time in col. P. M. to be $10^{h.}\ 30^{m.}\ 13^{s.}$, which, divided by 2 gives $5^{h.}\ 15^{m.}\ 6^{s}.5$, and which is the apparent time, it being in the afternoon.

The degrees and minutes corresponding to this cosine are 78° 46′, which is the sun's azimuth reckoned from the North.

When a star is observed, to find the apparent time and the azimuth.

RULE.—*Find the hour-angle by the preceding rule for the sun, taking it out from column P. M. and dividing by 2. The azimuth is this hour-angle turned into degrees, &c., and is to be taken from the top or bottom of the page, as the case may be. To the hour-angle add the star's right ascension, when west of the meridian, but take the difference between the hour-angle and the star's R. A., when it is east of it; the sum or difference will be the right ascension of the meridian. From the right ascension of the meridian (increased by 24 hours if necessary) subtract the sun's right ascension, the remainder will be the apparent time.*

Example II.

In latitude 35° N., when the sun's right ascension was $1^{h.}\ 4^{m.}$, and Aldebaran's right ascension $4^{h.}\ 27^{m.}\ 28^{s.}$, his declination 16° 12′ 32″ N., set the sextant at 16° 19′ 46″, having added 7′ 14″ for dip and refraction, and noted when the star reached that altitude, it being west of the meridian. Required the apparent time and the star's azimuth.

Dec.	16° 12′ 32″	log tang	9.46344
½ co-lat.	27° 30′ 0″	" "	9.71648
		Cos hour-angle	9.17992

	h. m. s.
Col. P. M.	2)10 50 22
Hour-angle	5 25 11
Star's R. A.	4 27 28
R. A. meridian	9 52 39
Sun's R. A.	1 4 0
Apparent time	8 48 39 P. M.

The azimuth corresponding to this hour angle is N. 81° 18′ W.

Having the hour-angle, to obtain the latitude.

RULE. — *Add together the log cosine of the hour-angle, and the log cotangent of the declination; the sum of these two logs will be the log tangent of an angle, which, when the latitude and declination are of the same name, being doubled and subtracted from* 90°, *will give the latitude; when the lat. and dec. are of different names, double the angle, and subtract* 90° *from it, the remainder is the latitude sought.*

Example III.

In latitude 35° 30′ N. by account, longitude 30° West from Greenwich, set the sextant at 26° 11′ 59″, and noted that the star Antares reached that altitude, March 5, 1853, at $4^{h.}\ 1^{m.}\ 20^{s.}$ A. M., apparent civil time; which is March 4, $16^{h.}\ 1^{m.}\ 20^{s.}$ apparent astronomical time. The star was east of the meridian, and its right ascension for that date $16^{h.}\ 20^{m.}\ 24^{s.}$; its declination, 26° 6′ 4″ S.

It will be perceived that I allowed 5′ 55″ correction for dip and refraction.

Turning the longitude into time, and adding to the apparent time, gives the Greenwich apparent time; for which time I take out the sun's right ascension, which completes the data necessary in the computation. Required the latitude.

	h.	m.	s.		
Apparent time	16	1	20		
Sun's R. A.	23	3	23		
	39	4	43		
Subtract	24	0	0		
R. A. meridian	15	4	43		
Antares' R. A.	16	20	24		
Hour-angle	1	15	41		
Multiply by			2		
Col. P. M.	2	31	22	log cosine	9.97588
Antares' Dec.	26°	6′	4″	log cotang	10.30988
	62°	37′	14″	log tang	10.28576
			2		
	125	14	28		
	90	0	0		
Latitude	35	14	28 N.		

The azimuth is the hour-angle turned into degrees, minutes, &c., and in this case is S. 18° 55′ E.

In Sumner's method, on observing a star, we have simply to use the assumed latitudes in formula (1), or (2), as the case may be, and which being computed twice, as in Example II., gives two points which are projected on a chart, and joined by a straight line. Another convenient star is observed as soon as possible, and two other points are obtained, which are also joined by a straight line; where these lines cut each other, will be the place of the observer.

To obtain the azimuth, introduce the true latitude, obtained by the projection, into the formula, and compute the hour-angle again in either case.

I will remark in conclusion, that amongst seamen there are many very expert observers, who are bad computers. This plan of observing will relieve them considerably.

In night observations there will be no difficulty in finding any number of stars suitable for observation; it is only necessary to be familiar with them, which any one in a short time will become, with the assistance of a good celestial globe, or charts of the heavens, with which, one or the other, every ship should be provided.

On board vessels of war, the master or navigator, besides his duties connected with the navigation of the vessel, is often a watch-officer. The writer has been called upon during his service to keep three watches, and write the smooth and rough logs, besides attending to the navigation of the vessel. Any seaman knows that under these circumstances very little idle bread is eaten.

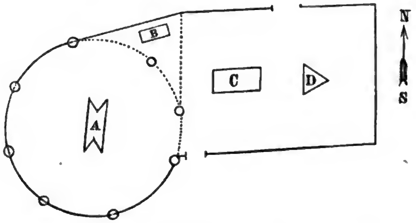
With this method, an officer so circumstanced, who has to take night observations, can take on deck with him, when he assumes charge of the watch, his sextant, almanac, and marking watch; before the four hours' watch is out, in clear weather, he will have had opportunities of taking a number of observations, the working up of which will not take up much of his time the next forenoon.

11. CLOVERDEN OBSERVATORY AND THE SHELBY EQUATORIAL. By DR. B. A. GOULD, JR. and PROFESSOR JOSEPH WINLOCK, of Cambridge.

HAVING felt the pressing need in the United States of more observations than are at present made, and an earnest desire to obtain the means of making a few ourselves, we have recently erected a small temporary structure at Cloverden in Cambridge, in order that such few hours as could be spared from our official and regular duties might be devoted to this purpose. From the nature of the case economy and simplicity were imperatively requisite, and as we have succeeded in constructing the necessary fixtures to our satisfaction, at a very small expense, we have thought that a brief allusion to the plan of the structure might not be without value in suggesting the small expense at which valuable astronomical instruments may be mounted, protected, and used. It may thus indirectly serve in aiding those to whom the construction of more permanent observatories is intrusted, where economy in building is important.

The College at Shelbyville, Kentucky, has, with signal liberality and a most gratifying confidence, intrusted to us for a definite period its exquisite equatorial telescope, constructed for that College, at Munich, by Messrs. Merz and Son, some four years since, and described by the makers in the thirtieth volume of the *Astronomische Nachrichten.* Our building was designed for the accommodation of this telescope, together with a transit-instrument, zenith-telescope, and clock.

The observatory, a ground plan of which is given in the accompanying figure, consists of a four-sided building 15 by 10 feet, represented by the parallelogram in the figure, communicating at its western extremity with a cylindrical structure 15 feet in diameter, which is covered by a revolving dome. The first-named portion is of the usual form and construction of the astronomical stations of the United States Coast Survey, one of which it constitutes. It is 6½ feet high at the eaves, and 9½ at the ridgepole. The four sides are built separately, each of vertical pine boards side by side, nailed to two parallel joists of 3 × 4 inch scantling, which, halved together at the ends, form the frame of the building, the roof resting upon the upper joist and the floor upon the lower. Wooden pins through the halved ends hold the four sides together. The ridgepole consists of a piece of similar scantling, bevelled at the upper corners, and cut away over the transit-instrument and zenith-telescope after the roof-boards and battens have been nailed on. Of course all the joints at the side and upon the roof are protected by battens, and the roof-boards tongued and grooved. Boards left loose, one on each side of the roof over the transit-instrument, and one on the north over the zenith-telescope, form the meridian apertures. They are kept in place by the battening on the outside, and long wooden buttons within, and may be set to any desired height by means of buttons at the eaves, which fit in notches made on the under side of the sliding board for the purpose. The trap-doors over the zenith-telescope are



each 18 × 10 inches, upon leather hinges to keep out the rain, and are supported and protected by battens at their edges. Cords from within, running over pulleys above, give the means of raising and lowering the doors. The cost of this portion of the building was $ 42. Being oriented with exact reference to the meridian, while the position of the cylindrical part of the building is to some extent limited by surrounding objects, the relative position of these two portions is not entirely symmetric, and a short side to the building became necessary toward the north-northwest, being tangent to the cylinder and forming a very obtuse angle with the north side. This circumstance, however, is far from being inconvenient, for it provides an admirable position (at B) for an astronomical clock. The wooden stand for the equatorial is shown at A, and the granite blocks for the transit-instrument and zenith-telescope are situated at C and D respectively, all three stands resting on the solid gravel three or four feet below the surface of the ground.

The transit-instrument and zenith-telescope were provided by the Coast Survey, for the determination of the latitude, and have respectively 46 and 43 inches of focal length. An extended series of observations has been made with them for the latitude, and promises, when fully reduced, to be of the highest importance, on account of the extremely discordant results of observations hitherto made in the vicinity.

The great principles indicated by modern practical astronomy as fundamental for the construction of observatories are, first, a rigid support for the instruments as near the ground as practicable; second, as slight a covering as is consistent with a proper security of the instruments, and this of such nature as to occasion the minimum difference of temperature between the external and the internal air. We think that these desiderata have been attained in the structure of this part of the edifice also, which is occupied by the Shelby equatorial. Cedar posts, about five feet apart, are set in the ground four feet deep in the circumference of a circle of eight feet radius. Upon these posts, seven feet from the ground, rests the ring upon which roll the balls that support the dome. This ring or plate, like the corresponding one which forms the base of the dome, is constructed of three thicknesses of inch pine board, six inches wide, sawed to the curve in short pieces and nailed together so as to break joint, and of a similar stratum of inch-and-a-half oak attached to these. The balls are retained in their way by long strips of pine cut so small as to bend readily to the curve, and fastened by rods to the inner and outer circumference of each ring. Rain and snow are excluded by a broad weather-board, bent in a similar way and secured to the exterior of the dome-plate. The six balls are like common tenpin balls, but turned from rock-maple instead of lignum-vitæ on account of the greater cheapness. The dome is a hemisphere, its skeleton formed of pieces of inch pine sawed to the same curve as those which form the plates, and nailed together, like those, to break joint. This is covered with cotton-duck, part of the topsail of an extinct schooner, soaked with a mixture of soft soap and white lead. The whole is turned with great ease by one hand, by means of a wooden handle attached to the dome-plate, and extending to within a convenient distance from the floor. An aperture of two feet is attained by placing the two central ribs parallel to one another. This extends two feet beyond the crown, two and a half feet at the bottom being closed by a door on hinges, and the remainder by a sliding shutter held in its place by a slight framework and readily controlled by cords and pulleys within. The cost of this second portion of the observatory was about $ 100. Were the building intended to be permanent, the posts should be placed somewhat closer together, and perhaps a more durable cover, say of tarpauling, or at least a double thickness of well-painted duck provided.

The Shelby Equatorial was made, as we have stated, for Shelby College in Kentucky, by Merz and Son of Munich, every part of it being finished in the best manner. The aperture of the object-glass is $7^{in.}.6$, and the focal length $10^{ft.}$ having $5^{in.}.7$, — the finder having about 2^{in} aperture and $21^{in.}.6$ focal length. The hour-circle is $10^{in.}.3$ in diameter, reading by two verniers to 4 seconds of time, the declination-circle $16^{in.}.3$, reading to $10''$ of arc. Sidereal motion is communicated by a friction clock.

There are five Huyghenian eye-pieces of the powers 102, 146, 232, 348, 550, and six micrometer eye-pieces of powers ranging from 100 to 580. The filar-micrometer was furnished by the makers with but two micrometer threads, but Mr. Wurdemann has inserted for us three others at right angles to these, and at least two more should be supplied parallel to the former, to prevent the screw from being too much used in measuring large intervals of declination. We have subjected the micrometer screw to most careful scrutiny, under differences of temperature amounting to more than 100° Fahrenheit, and rigidly tested it through its whole length. It is certainly of excellence entirely commensurate with the high reputation of its eminent maker. The position circle is four inches in diameter, graduated to 15 minutes, and read to 1′ by two verniers.

This beautiful instrument contains numerous improvements in the mounting, suggested by the experience of the artists and here introduced for the first time. Most of them are alluded to in the article of Mr. Merz already cited from the *Astronomische Nachrichten.* They are chiefly for the purpose of giving a more uniform equatorial motion by means of the friction-clock, and of correcting, to a more satisfactory degree than has hitherto been attained, for the effects of temperature on the declination-axis. The apparatus of the hour-axis has been simplified by dispensing with one set of friction-rollers, and substituting a lever and spring for the counterpoise to support the centre of gravity of the instrument; thus bringing the instrument, as its maker has said, "one step nearer to the fundamental idea of the equatorial." The micrometer-lamps have been improved by suspension in gimbals, and the clock, instead of being adapted to only one latitude, is adjustable over twenty degrees, so that the instrument may be employed in any places between 28° and 48° of North latitude.

We would, among these valuable improvements, especially signalize the beautiful precision with which the screw-threads of the hour-circle have been graduated and cut, and the convenient adjustments of the various double joints of the tangent screws, by which these may be used in any position for any hour angle or declination.

The object-glass is of rare excellence, unusually free from air-bubbles, and almost perfect in form, and has been successfully submitted to very severe tests. Saturn as seen through it exhibits with distinctness and precision all the ring phenomena as described by Struve, the dark ring being seen sharply defined at its interior limit, and of the same dimensions as observed by him and by Galle. The experience of Mr. Alvan Clark, of Cambridge, has supplied other test-objects in double stars of great difficulty, many of them discovered by himself with the noble lens which he has recently ground for Amherst College.

The Shelby telescope was purchased for the College by private subscription, raised by the President, Rev. William I. Waller, to whose exertions and liberality the attainment of so valuable an instrument for

Kentucky, is wholly due. In the absence of an observer at Shelbyville, the authorities of the College have kindly permitted us to remove it to Cambridge for a short time. It is to us a source of deep regret that our duties, demanding as they do constant attention, should deprive us of the power of devoting more time to the investigations for which it affords so desirable and excellent an opportunity. We trust, however, that, after its return to Shelbyville, it will not be suffered to remain idle.

Obstacles to the advantageous use of good instruments arise from the unfavorable position of Cloverden, which is not on high ground, thus rendering observations in the vicinity of the horizon difficult, if not impossible. One clump of trees especially, which we should not feel justified in removing unless we looked forward to a long continuance of our observations, cut off the view of the late comet (1854 II.) so effectually, as to prevent us from following this object so long, by at least ten days, as it was observed at Washington. Still, the possibility of observing it so long as we did through trees, under circumstances of such difficulty, is of itself a strong testimony to the excellence of the object-glass.

The following papers were read, but no copy of them has been furnished for publication: —

12. THE LONGITUDE OF AMERICA; DETERMINED BY MOON CULMINATIONS. By PROFESSOR BENJAMIN PEIRCE, of Harvard College.

13. COMPARISON OF THE BRITISH ASSOCIATION CATALOGUE OF STARS WITH THE GREENWICH TWELVE-YEAR CATALOGUE. By PROFESSOR ELIAS LOOMIS, University of New York.

14. THE ASTRONOMICAL EXPEDITION TO CHILI. By LIEUTENANT J. M. GILLISS, U. S. N.

15. ON THE PHYSICAL CONSTITUTION OF THE SUN AND COMETARY BODIES. By PROFESSOR W. A. NORTON.

16. ON THE SUPERIOR FACILITIES FOR THE COMPUTATION OF THE LUNAR EPHEMERIS AFFORDED BY A NEW SYSTEM OF ARGUMENTS INTRODUCED BY PROFESSOR PEIRCE INTO HIS "TABLES OF THE MOON." By J. D. RUNKLE.

17. ASTRONOMICAL DETERMINATION OF THE SUN'S DIURNAL AND ANNUAL INTENSITY. By L. W. MEECH.

II. MECHANICS AND PHYSICS.

1. On the Nature of Forces. By Lieutenant E. B. Hunt, Corps of Engineers, U. S. A.

It is a peculiarly significant fact, that all the great agencies of Nature which act from local centres or origins, through sensible distances, follow the Newtonian law of variation in intensity with an inverse duplicate function of the distance. In this respect light, radiant heat, sound, gravitation, electric and magnetic repulsions and attractions, all agree. As the so-called forces of cohesion, elasticity, chemical affinity, electrolysis, crystallization, capillarity, friction, &c. only exhibit sensible actions at insensible distances, their laws of variation in intensity with varying distances of action are incapable of direct determination, nor do we even know if these agencies are original and primary, or resultant and secondary forces. As inertia acts only in the ultimate units of matter, it cannot be supposed to be at all connected with distance. The mutual action of galvanic currents, in which both distance and inclination affect the attraction or repulsion, is clearly a complex result of the transmissive motion of the currents, and forms no static exception to the Newtonian law. It seems, therefore, a general fact, that all primary natural agencies which act from central origins through sensible distances are embraced under one mathematical formula, which is that expressed in the all-embracing Newtonian law.

What, then, is the rational translation or philosophical significance of this law? To this question one answer may be given which fully illustrates that crowning simplicity so uniformly characterizing natural facts of the highest generality. Newton's law rigorously expresses the necessary facts of simple outward emanation, or procession from a central origin. Whatever case of agency emanating from a centre we may suppose; whether it be light, radiant heat, sound, force, or any other, this fact of emanation makes the Newtonian law an inevitable result. This law simply expresses the fact, that the agency in question undergoes neither increase nor diminution by outward transmission. If any other than the inverse duplicate ratio be supposed, it must involve either an increase or diminution of the aggregate agency as consequent on mere transmission through space. But it is clear that mere transmission is totally incapable in itself of affecting in the slightest degree the quantity of action originally put forth from the centre. Mere change of place cannot, by its very nature, be a producing or destroying cause. The inertia, the structure, the imperfect elasticity of the transmitting medium, may produce a decay of transmitted action, as in the case of light in an imperfect medium, or of heat in air; but mere transmission as such is as wholly powerless to destroy as it is to create action.

The more clearly to perceive that unresisted central emanation necessarily gives the Newtonian law, let us conceive a centre from which action of any kind issues or emanates by rectilinear radiation. Each ray throughout its entire length is the representative of the same quantity of action. Now if we suppose elementary concentric spheres around this origin as a centre, each sphere is pierced by all the rays, and hence all the spheres become *loci* of the same amount of total agency in a given time. If the emanation or radiation be supposed uniform in all directions, then the total intensity of action on each unit of surface for any particular sphere is inversely as its total surface, which is as the square of the distance of transmission. Hence the action on a given surface, or a given constant mass, is inversely as the square of the distance of transmission. Or, instead of rays, we may suppose the emanation to proceed by spherical undulations, when a like train of reasoning will lead to a like result. The two mechanisms of radiation and of spherical undulation concur in giving the Newtonian law as the necessary expression of unobstructed emanation, the law being indeed but a simple assertion that the emanating agency is neither increased nor diminished by outward propagation, or that translation through space neither makes nor destroys light, heat, force, &c. The same facts in a negative order would characterize a central absorption of agency.

Since force emanation thus leads to the Newtonian law as a necessity, the reverse question arises, whether the existence of the Newtonian law does not of necessity involve emanation. It surely furnishes a powerful evidence of emanation, but is not a positive proof of it; for we can suppose the exact geometrical system of dynamic agency which emanation produces to be, by the original creation and constitution of matter, embodied in an identical static form. For instance, we may suppose an atom so constituted as to fill with its actual and organic self all the space to which its force action would extend, and thus to have everywhere a potentiality identical with that resulting from true emanation. This hypothesis literally makes each atom fill all space, and all atoms actually to coexist in each point of the universe. Thus, too, if one atom be moved by its centre, it must everywhere move through every other atom. We are mathematically compelled, either to admit this strange universal coexistence of al atoms in each point of space, each atom being everywhere truly distinct, or else to ascribe to an emanation from central points or nuclei all the forces which follow the Newtonian law. But the exceeding improbability of this coexistence theory may best be estimated by inquiring into the chance of an original static creation being based on the inverse duplicate ratio. *A priori* this particular ratio has no preeminence of probability over any *sub* or *super* ratio of variation. We may indeed ascribe the existence in nature of this exact ratio to an intelligent Divine choice or selection, but as a question of chances it is as infinity to one that some other ratio would have existed.

Between these two conceptions, each of which is a geometrical possibility, this consideration of chances almost compels us to choose the idea of emanation. When, too, we consider the exceeding complexity of mechanism and the great metaphysical difficulties involved in the idea of coexistence, and when we observe that the inertia of each atom appears thus to be diffused through the entire universe, the coexistence theory seems an hypothesis of the least promising character. For these reasons, the existence of Newton's law in any type of force or other agency seems legitimately and almost by constraint to be referable directly to the emanation and outward transmission of the force or agency from its originating central points or nuclei. As all known primary forces do in fact follow this law when acting through sensible distances, the inference follows that all these forces are actually emanative.

But are all primary forces necessarily emanative? Certainly not; yet we ought not, except as a last resort, to hypothecate forces *not* emanative, as such an hypothesis is unwarranted by our actual knowledge. All such hypotheses involve generic force types, unlike that one which includes all forces whose laws are really known to us.

Before assuming primary forces varying inversely with the first, third, or fourteenth powers of the distance, we are bound to exhaust all the resources for phenomenal interpretation offered by forces following the Newtonian law. To assume that the same primary force is attractive at one distance and repulsive at another, is like saying that *yes* becomes *no* by a change of latitude. The expedient of leading one primary force through various alternations of attraction and repulsion, as is apparently done in the theory of spheres of force, must to a reasoning mind appear too conveniently Protean and timeserving to be accepted as anything better than the fig-leaf of our ignorance.

We really know of but one type of force, and that one has a law which means emanation; yet speculation has run riot among all possible ratios of force decrease, and the force entity has been treated as a shuttlecock between attraction and repulsion, just as present convenience dictated. We must have a more grand and simple idea of force ere the labyrinth of molecular mechanics will yield its clew. In molecular studies, there is a strong and wide-spread tendency to complex hypotheses which but ill accord with the fundamental simplicity of Nature, and which, by hiding our ignorance, effectively retard our progress toward knowledge. To exorcise this tendency would greatly promote the consistent extension of strict mechanical investigation over the rich fields of molecular constitution.

With a view to developing the principles now presented, and as a preliminary to some discussion of the theoretical views advanced by Boscovich and Faraday, I will here proceed to develop a few of the properties of central forces, varying with an inverse function of the distance, and which may be either emanative or static by coexistence.

Assume a centre of force (or other agency) at an origin of rectangular co-ordinates, and conceive the force to be radiated uniformly in all directions, each ray being in its entire length the representative of a constant intensity of action, or of an agency varying in intensity with any inverse function of the distance. This mechanism must, it is evident, give results identical with those which would result from a corresponding spherical wave mechanism. Suppose now a circular disc to advance or recede relative to the origin by being moved along the axis of X by its centre, and being maintained perpendicular to it; the reception of rays by this disc will be a measure of effect so long as the obliquity of these rays can be disregarded. Calling the force or aggregate action y when the disc is at the distance x from the origin, and y' when it is at the distance unity, we have $y' : y :: x^2 : 1$, or $y = \frac{y'}{x^2}$. If now we conceive each ray as having an intensity varying with a simple inverse function of x, we shall obtain $y = \frac{y'}{x^n}$, in which n exceeds by two the exponent of variation along each ray.

If we differentiate the equation $y = \frac{y'}{x^2}$, regarding y as a function of x, we obtain $d\,y = \frac{-2\,y'\,d\,x}{x^3}$, and this is the force decrement corresponding to the Newtonian law. This can best be appreciated by deriving it directly. Let the disc advance towards the origin through a distance $= d\,x$; the increase of ray reception by the disc, or the differential of the force, is found by determining the elementary ring projected around the former position of the disc. Calling the disc radius r and the width of the added ring $d\,r$, we shall have by proportionality, $x : -d\,x :: r : d\,r$, and $d\,r = \frac{-r\,d\,x}{x}$; also,

$$y : d\,y :: \pi r^2 : \pi\,(r + d\,r)^2 - \pi r^2.$$

Hence by reduction and by neglecting $d\,r^2$ as an infinitely small quantity of the second order, we obtain

$$d\,y = \frac{-2\,y\,d\,r}{r} = \frac{-2\,y\,d\,x}{x} = \frac{-2\,y'\,d\,x}{x^3},$$

which is the expression above found as the differential of a Newtonian force. The signs of $d\,x$, $d\,y$, and $d\,r$ depend on the direction of the motion of the receptive surface.

It will be seen by inspecting the above, that not only is Newton's law derived from this consideration of ray reception, but that the differential equation of that law expresses simply the relation between the differential of distance and that of ray reception. This law also involves the two assumptions, which for all appreciable distances are entirely admissible, though not at all so for extremely small distances; first, that the effect of ray obliquity for the same receiving disc may be neglected; and, second, that the diffusion over the disc may be regarded as uniform. By substituting spherical atoms for the disc, we at once obtain the case of Nature when the question is of actions between sensible masses.

If we construct the curve of the equation $y = \frac{y'}{x^2}$, we find that both axes of co-ordinates are asymptotes to its branches, or that the force ordinate is infinite at the origin, and zero at an infinite distance. But the most remarkable fact in this class of curves is, that each particular radius of receiving disc corresponds to a particular curve. When the distance abscissa equals the disc radius, the differentials of force and distance are always equal in construction. The law of the increment is only satisfied by that curve which cuts the line through the origin, making an angle of 45° with the two axes, in the point whose ordinate equals the radius of disc. Thus the radius of the receptive disc or atom determines the particular characteristic curve of relation between force and distance, — a curve which is the same for a homogeneous sphere of atoms as for a single component atom.

Passing to the more general function, $y = \frac{y'}{x^n}$, and differentiating, we obtain $d\,y = \frac{-n\,y\,d\,x}{x^{n+1}}$, in which, if we substitute y for $\frac{y'}{x^n}$, we obtain $d\,y = n\,d\,x \frac{y}{x}$. If we suppose now that all the curves corresponding to any particular value of n are duly constructed, and if a right line through the origin make the angle a with the axis of x, then $\frac{y}{x} = \text{tang } a =$ a constant; or in $d\,y = -n\,d\,x\,\frac{y}{x}$ we find a constant of force increment for all the points in which a straight line from the origin cuts the various curves of the system. The significance of this result is obvious when we consider that radiation gives shape to the formula. By further discussion, it would be seen that the increment for a given ordinate varies inversely with the abscissas, and directly with the ordinates for a given abscissa.

If the series of parallel curves corresponding to $y = \frac{y'}{x^2}$ be constructed for all values of y' from plus infinity to minus infinity, any possible attraction or repulsion curve for which the force varies as $\frac{1}{x^2}$ will coincide with some one of this series. No two curves of this series, when referred to the same origin and axes, can be made to intersect. This property is general for all central force curves, in which y varies as $\frac{1}{x^n}$ taken in sets for each value of n from zero to plus infinity. This signifies that if any number of central forces acting from the same centre according to the same law are *in equilibrio* at one point, they must be so at all distances. Also, if any number of attractions and repulsions act from one point as $\frac{1}{x^n}$, their resultant also acts as $\frac{1}{x^n}$. Thus, $y_{a'} + y_{a''} + y_{a'''} + \text{\&c.} - y_{r'} - y_{r''} - y_{r'''} - \text{\&c.}$ $= y_a \text{ or } y_r = \frac{y'_{a'} + y'_{a''} + y'_{a'''} + \text{\&c.} - y'_{r'} - y'_{r''} - y'_{r'''} - \text{\&c.}}{x^n}$ $= \frac{y'_a \text{ or } y'_r}{x^n}$, which is of the original form. Hence all forces emanating from a centre and varying as $\frac{1}{x^2}$ are equivalent to a single resultant varying as $\frac{1}{x^2}$, which is wholly attractive, or wholly repulsive; and similarly for each value of n. Hence a medium composed of one species of atoms, endowed with forces varying according to Newton's law, must be unalterably compacted or unalterably elastic. Therefore, a single type of atoms with Newtonian forces cannot represent the facts of nature, and we must either suppose other forces than those varying as $\frac{1}{x^2}$, or we must suppose more than one kind of matter.

If we suppose an atom to exercise two central forces, one attractive and one repulsive, for which n has different values, their curves will intersect, or the forces will balance at one, and only one, distance. Thus, if the ordinary attraction $y_a = \frac{y_{a'}}{x^2}$ and any repulsion $y_r = \frac{y_{r'}}{x^n}$ act from the same centre, their resultant $y_a - y_r = \frac{y_{a'} x^{n-2} - y_{r'}}{x^n}$ can only be zero when $x =$ infinity, or when $x = \sqrt[n-2]{\frac{y_{r'}}{y_{a'}}}$. In general, $x = \sqrt[n'-n'']{\frac{y_{r'}}{y_{a'}}}$ is the abscissa or radius of the point or sphere of equilibrium of these two forces. Hence such a primary attraction and a primary repulsion acting between simple atoms can, together, give but one type or form of equilibrium, and thus must fail to give the solid liquid and gaseous conditions of aggregation. Besides, two forces involving different laws of action or values of n can in no wise give the simple Newtonian form as it appears in gravitation. From this we can say with confidence, that heat is no mere radical atomic repulsion varying as $\frac{1}{x^n}$.

To suppose three or more distinct atomic forces varying as $\frac{1}{x^{n'}}$, $\frac{1}{x^{n''}}$, and $\frac{1}{x^{n'''}}$, or to suppose a single force following no simple functional law, but being now repulsive, now attractive, now infinite, and again Newtonian, is to give ourselves up to bewilderment, and to achieve a chaos of explanation. Simple emanation and its resulting law bear *a priori* credentials of being the great facts of dynamic nature, and until they are shown to fail entirely in the exposition of molecular mechanics, a resort to other conceptions must be considered illogical and unhopeful. The hypothesis of two distinct species or kinds of matter with original forces peculiar to each, but all following the Newtonian law, is far more promising than any other, and Mossotti has, to some extent, shown its ample power of explanation. One important element is overlooked in his analysis, nor has he developed the mechanical effects of heat on aggregation, though in this respect his views are a vast advance on the total neglect of heat by Boscovich's theory, as also on the expositions by Poisson.

From the point which this discussion has now reached, I wish to examine somewhat the theory of Boscovich and the speculation of Faraday, freely stating some objections not hitherto urged.

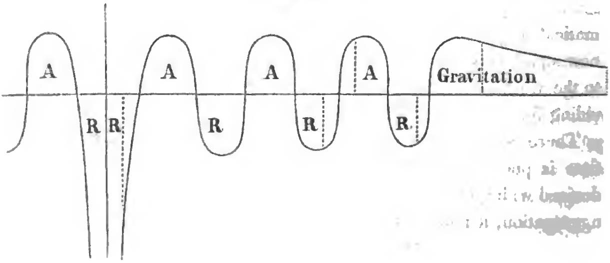
Boscovich, by denying size to atoms, prevents them from presenting any material surface or volume on which to receive force action. If forces emanate from the atoms of a mass A and penetrate the volume of a mass B, the atoms of the mass B would not, according to the Boscovich theory, present any surface or volume whatever on which to receive the force action. How, then, is the Newtonian law, or indeed any action, to be derived? As this law but expresses the condition of ray reception, what does it mean when this reception is precluded by the total lack of magnitude in atoms? Nothing remains but to conceive the force rays or their equivalents from the atoms in B, as everywhere receiving the action of the intersecting rays from the atoms in A, and referring these actions back to their own centres. Now, unless these rays are conceived as possessing magnitude, and as actually filling all space, the result of ray-intersections, depending as it must on the number of intersections, would not be the exact Newtonian law, but one essentially departing from this, by a difference which increases as the rays from each atom are supposed less completely to fill all space. Thus, to obtain the Newtonian law, we appear to be driven again to that strange hypothesis of each atom filling all space, and all atoms coexisting in each point, and to require still other special conditions: all simply as a consequence of denying size to atomic nuclei. As the power of receptivity must exist either in atoms of finite size or in atoms of infinite size, and as we must either locate inertia in a nucleal atom or in an infinite one, we seem quite justified in preferring emanation from and reception by definite nucleal atoms to the bold hypothesis of a static entity, activity, and inertia belonging to infinite coexistent atoms. If we attempt to conceive a material mass, as a wall for instance, according to this coexistence theory, we shall find it signally inadequate for the realization of facts to the mind; which, though not a logical objection, is a serious practical drawback. But by conceiving atoms as solid, impenetrable, definite volumes, from which force incessantly emanates and by which force is incessantly received, the mental difficulties wane away, and matter becomes to the mind a localized reality. However small the atomic volumes be assumed, so long as they have a real and finite size, a receptive capacity and the Newtonian law result at once.

If I rightly apprehend Faraday's views (Phil. Mag., Nos. 157 and 188), they are such as would give a law quite different from Newton's. The interactions of rays conditioned as he supposes could only give the actual result, by so extending the amplitude and number of rays as that all points of space should be points of interaction between the rays of each atom and of all other atoms, which is the coexistence theory again. To deduce the actual law from the views so modestly set forth by this excellent investigator would, I think, be a mathematical impossibility; to say nothing of their inadequacy, as they now stand, to serve the cause of molecular mechanics. The objection to the views of Boscovich and Faraday, on the ground of their not providing for inertia, has been well urged by Airy (Phil. Mag., No. 190).

There is another signal fault of the Boscovich theory, which at this time is peculiarly objectionable. While its mechanism is empirically devised with special reference to the solid, liquid, and gaseous states of aggregation, it really takes no account of heat, but at once refers these states to primary forces assumed for the purpose. Yet it is certainly the degree of heat, and that alone, which in fact mainly determines these forms of material existence. If any relation is supposable between heat and the force spheres of Boscovich, it remains to be discovered what it may be. But as the theory now stands, heat is ignored, and force spheres usurp the work actually performed by heat. This theoretical false causation is a positive stumbling-block in the way to clearer views of heat and molecular aggregation. The more we reflect on the wide range of actions due to heat, the more incompetent to their representation shall we find the conception of primary spheres of force. In nature, aggregation is actually almost absolutely ruled by temperature, yet nothing at all like temperature seems legitimately deducible from the Boscovich theory. This theory, indeed, requires a mass of any given substance, when not compressed, always to fill exactly the space, given by its atoms being in positions of mutual indifference, and does not provide for expansions and contractions through heat variations.

I will now state a striking proof, that the views of Boscovich have received their present somewhat wide acceptance among men of science without being subjected to that criticism, analysis, and comparison with nature, which alone can entitle them to any authority or weight. There is a glaring fundamental oversight involved in his theory as it now stands, and as it is figured in that exponential curve so frequently found in works on this subject, and even in mechanical text-books. This oversight consists in his not having correctly extended his theory to masses of matter, which, unfortunately for his theory, is the only case occurring in nature. Taking as it stands his exponential curve of force between two atoms, (Fig. 1,) and discussing

Fig. 1.

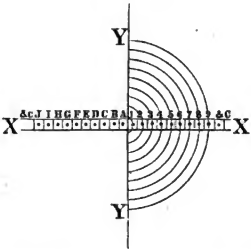


a mass composed of such atoms, it appears that, instead of the various types of aggregation and force manifestation hitherto supposed to result, there will be but a single cohesive type which will be invariable for each mass.

To illustrate this, let us assume along the axis of X (Fig. 2) a line or thread of atoms at mutual distances corresponding to a particular solid or liquid, and acting on each other according to the Boscovich force curve: call the atoms to the left of the origin A, B, C, D, &c., and those on the right, 1, 2, 3, 4, &c. In volume one of Robison's Mechanical Philosophy, there is an exposition of Boscovich's theory to which most of his disciples are indebted for their acquaintance with his views, and in this the widest limit of cohesion is fixed at about one thousandth of an inch, within which are several alternations of attraction and repulsion branches. Now, by comparing this distance with the almost infinitely minute threads and membranes, eye-points, &c. revealed by the microscope in infusorial and other organic forms, or with any of the countless facts showing the extreme divisibility of matter, and the differential character of inter-atomic distances, it will become evident that many thousands of atoms lie within this outer limit of cohesion, or in the one thousandth of an inch measured in a mass. Boscovich, in his *Theoria*, leaves the case essentially in the same condition. Hence the atoms A, B, C, D, &c. on the left of the origin act on 1, 2, 3, 4, &c. on the right of the origin, the attractions and repulsions alternating through many thousands of atoms on each hand. The atom A repels or attracts 1, 2, 3, 4, &c., according to distance, all atoms exterior to the last limit of cohesion being attracted. Thus, in the force curve for A, many atoms will be found to correspond to each branch. We may, therefore, with almost perfect accuracy, regard the areas between the several branches and the axis of X, as proportional respectively to the corresponding aggregate actions of A on 1, 2, 3, 4, &c. The action of B on 1, 2, 3, 4, &c. is analogous, and so of C, D, &c., a portion of the curve towards the origin being cut off for each receding atom. Now, on examining the areas between the different branches of the curve as drawn in Boscovich's Theoria, Robison's Mechanical Philosophy, Daubeney's Atomic Theory, Bartlett's Mechanics, &c., it will be seen that the outermost area corresponding to gravitation is very much greater than that for the adjacent repulsion branch, and that the successive attraction and repulsion branches embrace about equal areas. Hence the attraction between A and the gravitating atoms in the line is decidedly greater in its aggregate than the sum of the adjacent repulsions; as is amply realized when we consider that the attraction area extends to infinity between the curve and the asymptote axis. Hence the action of A would be to draw the gravitating part of the column towards itself with a much greater force than it repels the adjacent portion, so that a large surplus of attractive pressure is passed along the column to the next attractive branch, which is thus made greatly to surpass the next repulsion, and so on through the whole curve until the interior repulsion is reached, when the aggregate attractive surplus is balanced by the final indefinite repulsion. The action of B, C, D, &c. is entirely similar, except in the successive pruning of the inner extremity of the curve. Hence the aggregate action of A, B, C, D, &c. on 1, 2, 3, 4, &c. is simply a prevailing attraction, which is only effectively resisted by the final indefinite repulsion; and thus the whole mechanism of this curve serves only to make some perturbations in attraction, with no palpable result whatever.

Passing now from a line of atoms to a medium or mass of matter, the same result is found, only vastly exaggerated. Referring the medium to three rectangular axes of X, Y, and Z, and conceiving the line of atoms already discussed as coinciding with the axis of X, (Fig. 2,)

Fig. 2.



we wish to determine the aggregate forces which counteract each other in a superficial unit of the plane Y Z. The total action of the column A, B, C, D, &c. on the matter filling the space beyond the plane Y Z, affords the true criterion of forces acting in a medium, since all the columns parallel to A, B, C, D, &c. can be similarly treated, and thus all forces acting through the plane Y Z will be included in the discussion.

To realize the action of A on the mass beyond the plane Y Z, construct, with A as a centre, consecutive spherical surfaces through atoms 1, 2, 3, 4, &c. to infinity. The quantity of matter in these hollow shells increases as the square of the radius: hence the aggregate force exerted by and on a shell involves a particular function of the square of the distance. The result already found for the action of A on the line of atoms 1, 2, 3, 4, &c. must therefore be multiplied by this function of x^2 to obtain the action of A on the series of spherical shells. For the gravitation branch of the curve the direct and inverse functions of x^2 will neutralize each other, and the curve will become a straight line parallel to the axis of X, giving an infinite aggregate attraction for an infinite medium. In other words, all gravitating shells give equal total attractions. For the atoms B, C, D, &c. nearly the same result will be found, by a like process, the only difference being in the cutting off portions from the origin end of the curve. The total action of A, B, C, D, &c., as of all the parallel atomic columns, will thus be to give this measureless preponderance to the attractions. Hence a medium composed of atoms acting on each other according to the Boscovich force curve would be unalterably cohesive; and the effects of the various attraction and repulsion branches, interpolated between gravitation and the final repulsion, would be totally insignificant.

In an indefinite medium the gravitation area would greatly exceed the area of the final repulsion, so that such a medium would, of its own accord, rush in upon itself and become, as it were, but a single gigantic atom with a definite atmosphere surrounding it.

Whoever will follow this simple exposition of the effects of Boscovich's exponential or experimental curve of inter-atomic forces when such atoms constitute a medium or mass, which case alone exists in nature, will see that this curve and the theory it expounds are deplorably false to facts, and destructive to their own pretensions. An exponential curve might doubtless be devised, which would provide for the function of the square of the distance, and which would obviate the present gross exaggeration of attraction, though such an empirical curve seems little worth the labor.

Surely, however, it is time for all to discard that misshapen and self-destroying exponential curve which has too long passed unchallenged, because Boscovich was a really great mathematician, — great enough, indeed, to have fully recognized the faults of his theory had they been actually pointed out to him. It is a strange oversight on his part that he did not perceive the fallacy involved in his process of first constructing four atoms into a particle, four particles into a particle of the second order, &c., as if his primary forces would recognize the ideal boundaries of such particles. He in fact neglects all actions except those between adjacent particles when he passes to a medium, and Robison most pointedly does the same, in his favorite conception of springs uniting atoms. If this neglect were really meant, the question would arise as to what becomes of the machinery for gravitation, and what springs are those binding the sun and earth? I cannot but regard the processes of reasoning employed by Boscovich and Robison as singularly rude and gross for such men and such a subject: nor can molecular mechanics receive a more needful service than by the expurgation of views so abounding in error, and so obstructing the pathway to light. A fabric of objections and difficulties will surely arise in the mind of any well-furnished investigator, who will really think strictly on this renowned theory of spheres of force. The objections now presented are but specimens.

The speculation of Faraday lacks the definiteness of Boscovich's theory, and is not pushed into the field of molecular aggregation, nor indeed could it be with much hope of success.

Ray-vibrations would be a very ingenious mechanism for gravitation if the Newtonian law could be deduced in a tolerably simple manner from it, but this requisite seems to throw us back on the strange theory of a universal coexistence of all matter. Its inaptness for illustrating molecular mechanics is peculiarly striking, if we attempt to imagine ray-vibrations for the several phases of molecular constitution. In fact, the reduction of all forces to one law such as that of Boscovich or Faraday is like describing all animals as of the color of a chameleon.

In strange contrast with the Theoria and the "Speculation" is the investigation by Mossotti, which is based on real mechanical principles, and which, though quite imperfect, leads to real results. By assigning definite size to atoms, and applying the simple Newtonian law of force to two kinds of matter, conditioned as in the Franklinian electrical theory modified by Epinus, Mossotti has avoided most of the objections urged against the theory of spheres of force, and has given a glimpse at least of what heat is, in the constitution of masses. By extending his investigation, and by supplying some deficient elements, molecular mechanics may at last be established on that simple and sure basis of ordinary mechanical principles, which Newton and La Place have so distinctly foreshadowed, and which the expanding realms of physical science now demand with a positiveness hitherto unknown.

2. Note on a New Electro-Chronometric Method. By Professor Wolcott Gibbs, of New York.

The methods which have hitherto been employed for the measurement of small intervals of time by means of electric currents, are five in number. In the first of these methods, a cylinder is made to revolve uniformly round its axis, so as to perform a definite number of revolutions in a known interval of time. If we now suppose the armature of an electro-magnet to be attached to a lever beam in such a manner that the passage of the current shall cause the extremity of the lever to press against the surface of the cylinder, it is clear that a line will be described upon the surface of the cylinder, the length of which will bear the same proportion to the circumference of the cylinder, which the duration of the electric current bears to the time of revolution. By this method, then, we reduce the measurement of time to the measurement of space.

The second method is, in principle, similar to this. A fixed metallic point is brought near the surface of a polished metallic cylinder, which revolves uniformly as in the last case. The passage of an electric spark between the point and cylinder gives a black spot, and the space between two successive spots measures the interval of time between the passage of two successive currents. This method is due to Siemens, and has been successfully employed in experiments on the velocity of projectiles.

In the third method, the surface of a revolving cylinder, or disc, is covered with chemically prepared paper. The passage of the current produces chemical decomposition wherever the point of the conducting wire touches the paper, and thus the interval which elapses during the passage of the current, or between the passage of two successive currents, is registered by a colored line. This method, as well as the first mentioned, has been employed in this country in the operations of the Coast Survey, and, I believe, in astronomical observations.

The fourth method is that of Pouillet. In this the effect of the electric current upon the vibrations of a magnetic needle is carefully observed, in such a manner that the differences in the arcs of oscillation can be converted into differences of time. By observing the vibrations of the magnetic needle by the refined methods introduced by Gauss and Weber, Helmholtz has brought this method to a high degree of perfection.

The fifth method consists in converting the measurement of time into that of space, by means of the revolving mirror as employed by Wheatstone. The advantages and disadvantages of these various methods have been frequently discussed, and are too well known to require consideration in this place. I venture to bring forward still another method, in the belief that it also has its advantages, and that in particular cases it may possibly be preferable to other and more elaborate devices. The experiments of Wheatstone long since showed that the duration of the electric spark is but a very small fraction — less than the millionth — of a second. They showed at the same time that the eye clearly perceives and distinguishes objects which are illuminated for only this very small interval of time, so that a wheel in rapid rotation, during the passage of the spark, distinctly appears at rest. It is this principle which I propose to apply to the measurement of small intervals of time. Let us suppose the second-hand of a clock removed, and in its place a wheel, revolving once in a second. If the circumference of this wheel be divided into 1,000 equal parts, and if each of these parts be numbered in regular order from 1 to 1,000, each division with its number will pass before the eye, when properly placed, in the thousandth part of a second. If we now place before this revolving second-wheel an opaque diaphragm, having in it a small opening sufficiently large to permit one or two of the divisions on the circumference of the revolving wheel to be seen at once, and if the terminal wires either of a Leyden jar or of an electro-magnetic helix be placed in front of the opening, we have all that is necessary to enable us to measure the thousandth of a second, or the number of thousandths of a second, which have elapsed between two successive passages of the spark. The observer has only to note the number which appears upon the circumference of the wheel. At the instant the spark passes, this number will appear perfectly distinct, and may be read off with ease. The clock itself gives the minutes and seconds, directly, or the seconds may be counted in the usual manner, by the beats of the pendulum. It is obvious that, if necessary, the circumference of the second-wheel could be divided into a greater number of parts, and thus still smaller fractions of a second be registered, or, the number of divisions remaining the same, the number of revolutions of the wheel in a second would be increased; by either of these methods, the ten-thousandth, or even a still smaller fraction of a second, could be measured. Finally, for the convenience of observation, two or more wheels could be used, the first giving the seconds and tenths, the next the hundredths and thousandths, and so on. The figures could be placed either on the face of the wheel or on its rim, and, if necessary, a telescope might be employed in observation. In the form here suggested, the instrument would not, of course, be self-registering. Should it be found, however, to work well in practice, this disadvantage could easily be remedied, since the researches of Fox Talbot have shown that an object illuminated by an electric spark is yet capable of impressing sensitive paper with a perfectly formed image. It would then only be necessary to place a camera obscura in front of the clock, and receive the images upon a revolving cylinder of prepared paper, and we should have an automatic register. The apparatus here suggested might be employed in observations for the determination of longitude, but would be liable to the same sources of error as the methods which have been used by the Coast Survey; to the errors arising, namely, from the want of perfect uniformity in the revolution of the cylinder, from armature time, and, in astronomical observations, to those which arise in addition from the personal equation of the observer, which, as Dr. Gould has shown, are not constant even for the same observer, and during the same evening.

On the other hand, it appears probable that the proposed method would give good results, wherever short circuits can be employed, and wherever at the same time — as in the case of the motion of projectiles, or of falling bodies — the circuit can be made or broken without the direct intervention of the observer. Helmholtz, in a most valuable memoir on the rate at which intelligence is transmitted to and from the brain, by means of the nerves, has demonstrated that from three twentieths to three tenths of a second elapse before an impression can be conveyed to the brain, the will be exercised, and the muscle be moved. These results, taken in connection with those of Dr. Gould already alluded to, render it, in my judgment, very doubtful whether the ingenious instruments which Saxton, Mitchell, and others have devised for the more accurate measurement of time in astronomical observations, are capable of giving with accuracy intervals of less than the tenth of a second. The instrument which I have proposed is, of course, no better for this class of observations.

3. ABSTRACT OF A PAPER ON THE INVERTED MICROSCOPE; WITH SOME REMARKS ON THE ILLUMINATION OF MICROSCOPICAL OBJECTS. By PROFESSOR J. LAWRENCE SMITH, of Louisville, Ky.

As the principle of the instrument was described at the Albany meeting, no details on this head were entered into, but all the additional improvements were described, embracing points altogether connected with the illumination; which has been so much improved as to surpass all other forms of instruments for producing the best effects with the same glasses. The paper also insisted on the superiority of direct illumination from any illuminating source (when sufficiently intense) without the intervention of either lenses or reflectors. A new variety of oblique condenser was explained, consisting of a large plano-convex lens and the half of a small plano-convex achromatic lens so placed that the axis of the tube containing them was oblique; the effects of this arrangement are better than those of any other form yet tried by the author.

THE following papers were read, but no copy of them has been furnished for publication: —

4. ON THE ARRANGEMENT OF LECTURE-ROOMS, WITH REFERENCE TO SOUND AND SIGHT. By PROFESSOR JOSEPH HENRY, Secretary of the Smithsonian Institution, Washington.

5. ON IRRADIATION. By PROFESSOR W. B. ROGERS.

6. ON THE RESISTANCE EXPERIENCED BY BODIES FALLING THROUGH THE ATMOSPHERE. By PROFESSOR ELIAS LOOMIS, University of New York.

7. ELECTRIC PROPERTIES OF WHALEBONE RUBBER. By JOHN M. BATCHELDER. (Communicated by Professor B. Peirce.)

8. ON A NEW FORM OF ELECTRICAL MACHINE. By G. C. SCHÆFFER.

III. PHYSICS OF THE GLOBE.

1. PRELIMINARY DETERMINATION OF CO-TIDAL LINES ON THE ATLANTIC COAST OF THE UNITED STATES, FROM THE COAST SURVEY TIDAL OBSERVATION. By A. D. BACHE, Superintendent. (Communicated by Authority of the Treasury Department.)

IN the progress of the hydrography of the Atlantic coast of the United States, numerous tidal observations have been necessarily made for correcting the soundings and determining the establishments of the ports. With these I have connected observations of a more permanent character intended to furnish the data for ascertaining the laws of the tides in important localities, and others for tracing the progress of the tide-wave along the coast generally, and, in especial cases, in sounds, bays, and rivers. These observations are still in progress; indeed, those for developing the laws of the tides, and determining the constants of theory, depend for their value upon their long continuance. So many authentic results have now, however, been obtained, that it appears desirable to put them together, and to ascertain the conclusions towards which they tend as to the co-tidal lines, and by the agreement of the separate results with general laws, or their departure from them, to determine which of the cases require further observations to check these first results, and where new stations of observation are necessary for the purpose. My attention has been called also, by the request of a valued friend, the Master of Trinity College, Cambridge, to some attempt of this sort, and his labors in connection with this subject on our own coast have entitled his request to the most respectful consideration. I am indebted to L. F. Pourtalès, Esq., in charge of the tidal party of the United States Coast Survey, for the revision of the computations given in this paper. The labor of reducing the observations themselves has fallen chiefly upon Messrs. Heaton, Fendall, and Hawley, and the diagrams have been prepared by Mr. C. Fendall, under the direction of Mr. Pourtalès. The stations at which observations of the tides have been made, of the more reliable class, are thirty-three in number, extending on the Atlantic coast from Cape Florida to Portland, Maine. I have been able, through the kindness of Captain Shortland, of the Royal Navy, in charge of the Admiralty Survey of Nova Scotia, to extend the results to the entrance of the Bay of Fundy.

Table No. 1 gives the names of the places of observation, with the time during which the observations of high and low water were made, and remarks in relation to them. The stations marked * have been made use of in determining the co-tidal lines for this paper.

A few stations have been embraced in the results where the observations are not as numerous as those at the other points, chiefly to introduce localities important in position, and to sift the observations already made at them. Old Point Comfort, New York, and Boston Harbor have been permanent stations for some years; Charleston, Tybee Entrance, Portsmouth, and Portland have been more recently added to them.

To the short series of observations especially, there should be applied corrections for declination and parallax; but after computing several cases I was satisfied that the errors from other causes, and especially from the positions almost indispensably necessary to the tide gauges, more than made up for any irregularities from this source, and determined in this preliminary inquiry to omit these corrections, which amount only to a few minutes even in extreme cases.

A much more important correction is that for the position of the gauge in a harbor or river entrance, in many cases within a bar. Where our charts are completed, we have the elements for computing this correction by the law of depth, supposing the wave to move in the channel with a velocity proportional to the square root of the depth. This law, when applied to two very different cases, Savannah River and Boston Harbor, where we had the means of testing it by measured distances and known depths, was so completely verified, that I have not hesitated to apply it in the other cases.

TABLE NO. I.

Stations.	No. of Obs. Lunations.	Years.	Remarks.
*Cape Sable, N.S.,			Furnished by Capt. Shortland, R.N.
*Ellenwood's Isl., N.S.,			" " " "
*Fourchue Isl., N.S.,			" " " "
*Portland, Maine,	6½	1852, 3	Self-registering tide-gauge.
*Portsmouth, N.H.,	11	1851, 2, 3	Self-registering in 1852 and 1853; [all the sets agreeing pretty well.
*Newburyport, Mass.,	2	1851, 2	
Ipswich,	2	1852	
*Gloucester,	1	1853	
*Salem,	2¼	1850	
*Nahant,			
Boston Light,	3½	1847	Results reliable.
*Boston Dry Dock,	6½ yrs.	1847 to 1853	Best series.
*Wellfleet,	1	1849	
*Provincetown,			From Major Graham's survey.
Monomoy,	1½	1852	
Great Point, Nantucket,	1½	1849, 50	
Siasconsett,	3	1853, 54	
Nantucket Harbor,	13	1846, 7, 8, 9, 50	Irregular.
Tuckernuck,	1	1850	
Wasque Point,	1	1853	
Edgartown,	7	1846, 7, 51, 52	
Holmes' Hole,	6	1846, 51, 52	
Wood's Hole (East),	3	1849, 52	
Tarpaulin Cove,	3	1849, 51	
*Quicks' Hole (South),	2	1851	
Menamsha Bight,	1	1852	
*Fort Adams, Newport Harbor,	16	1844, 5, 6	
*Point Judith,	6	1844, 5	
Watch Hill,	2¾	1848	
Montauk Point,	2½	1848	Not a good series.
Fire Island,	2	1850	Ditto.
*Sandy Hook,	17	1835, 36, 44, 45, 46	
*Cold Spring Inlet,	11	1843, 44	Day tides only.
Cape May Landing,	2	1847	
*Delaware Breakwater,	10½	1840, 1, 3, 7	
*Old Point Comfort,	65	1846 to 1851	
*Hatteras,	1	1850	
*Beaufort, N. C.,	2		
*Cape Fear Entrance, Smithville, N. C.,	9	1851, 2	
Georgetown, S. C.,	1½	1852	
*Charleston,	6	1853	
*Fort Pulaski, Tybee Entrance,	13	1851	
*St. John's River,	1	1853	
*Cape Canaveral,	11 tides	1850	Quite unreliable.
*Cape Florida,	1	1852	

TABLE NO. II.

Co-tidal Hours of Ports on the Atlantic Coast.

	Stations.	Corrected Establishments.	Longitudes from Greenwich.	Corrected Establishments in Greenwich Time.	Corrections.	Co-tidal Hours.	Co-tidal Hours carried to Coast by correcting for Depth of Water.	Latitudes.
		h. m.	h. m.	h. m.	m.	h. m.		° ′
1.	*Cape Sable,	8 50	4 23	13 13	18	12 55		43 25
2.	*Ellenwood's Island,	9 45	4 24	14 9	19	13 50		43 39
3.	*Fourchue Island,	10 0	4 25	14 25	20	14 5		43 47
4.	*Portland, Maine,	11 25	4 41	16 6	23	15 43		43 35
5.	*Portsmouth, N. H.,	11 23	4 42	16 5	23	15 42		43 3
6.	*Newburyport, Mass.,	11 22	4 43	16 5	23	15 42	15 27	42 48
7.	Ipswich, Mass.,	11 32	4 43	16 15	23	15 52		42 41
8.	*Gloucester, Mass.,	11 10	4 43	15 53	22	15 31		42 37
9.	*Salem, Mass.,	11 13	4 43	15 56	22	15 34	15 19	42 31
10.	*Nahant,	11 12	4 43	15 55	22	15 30		42 25
11.	*Boston Light,	11 12	4 43	15 55	22	15 33		42 20
12.	*Boston Dry Dock,	11 22	4 43	16 5	22	15 53	15 30	42 20
13.	*Wellfleet,	11 5	4 40	15 45	22	14 43		41 56
14.	*Provincetown,	10 35	4 41	15 16	21	14 55		42 3
15.	Monomoy,	12 5	4 40	16 45	24	16 21		41 36
16.	Great Point,	12 4	4 40	16 44	24	16 20		41 23
17.	Siasconset,	11 53	4 40	16 33	24	16 9		41 15
18.	Nantucket,	12 11	4 40	16 51	24	16 27		41 17
19.	Tuckernuck,	11 35	4 41	16 16	23	15 53		41 18
20.	Wasque Point,	9 19	4 42	13 1	19	12 42		41 21
21.	Edgartown,	11 6	4 42	15 48	22	15 26		41 24
22.	Holmes' Hole,	11 43	4 42	16 25	23	16 2		41 28
23.	Wood's Hole (East),	8 12	4 42	12 54	16	12 38		41 31
24.	Tarpaulin Cove,	8 4	4 42	12 46	16	12 30		41 28
25.	*Quicks' Hole (South),	7 32	4 42	12 16	15	11 59		41 26
26.	Menamsha Bight,	7 52	4 43	12 35	16	12 19		41 20
27.	*Fort Adams, Newport Harbor,	7 45	4 45	12 30	15	12 15	12 4	41 29
28.	*Point Judith,	7 32	4 45	12 17	15	12 2		41 22
29.	Watch Hill,	9 0	4 47	13 47	18	13 29		41 19
30.	Montauk Point,	8 10	4 46	12 56	16	12 40		41 4
31.	Fire Island,	7 18	4 52	12 10	15	11 55		40 38
32.	*Sandy Hook,	7 29	4 56	12 25	15	12 10		40 28
33.	*Cold Spring Inlet,	7 32	4 59	12 31	15	12 16		38 57
34.	Cape May Landing,	8 19	4 59	13 18	17	13 1		38 56
35.	*Delaware Breakwater,	8 0	5 0	13 0	16	12 44		38 48
36.	*Old Point Comfort,	8 17	5 6	13 23	17	13 6	12 3	36 55
37.	*Hatteras,	6 57	5 2	11 59	14	11 45		35 15
38.	*Beaufort, N. C.,	7 40	5 7	12 47	15	12 32	12 18	34 37
39.	*Smithville, N.C., Cape Fear Entrance,	7 19	5 12	12 31	15	12 16	11 40	33 54
40.	Georgetown, S. C.,	7 52	5 16	13 8	16	12 40		33 22
41.	*Charleston (Castle Pinckney).	7 13	5 19	12 32	14	12 18	11 38	32 43
42.	*Fort Pulaski, Tybee Entrance,	7 20	5 23	12 43	15	12 28	12 0	32 1
43.	*St. John's River,	7 31	5 26	12 57	15	12 42	12 27	30 20
44.	*Cape Canaveral,	7 21	5 22	12 43	15	12 28		28 27
45.	*Cape Florida,	8 26	5 20	13 46	17	13 29		25 40

The preceding table, No. II., contains in the first column a number for reference; in the second, the names of the stations; in the third, the mean luni-tidal intervals or establishments; in the fourth, the longitude from Greenwich; in the fifth, the approximate co-tidal hour, obtained by adding to the establishment the difference of longitude; in the sixth, a correction of one minute for every half-hour in the establishment, to correct for the different transits of the moon used in reducing the observations (see Mr. Whewell's paper, Phil. Trans., 1836, p. 293); in the seventh, the co-tidal hour thus corrected; in the eighth, the co-tidal hour corrected for depth, where data were at hand for the purpose; in the ninth, the latitude of the station.

In order to obtain the best results from the observations, they have been divided into groups, in the way and from considerations which will be hereafter explained. Supposing the stations to be really connected in the same physical group, and assuming that the space over which the observations extend is such that the co-tidal lines may be taken as straight lines, we obtain by least squares the position of the co-tidal lines which will best satisfy the equations. The mean latitude and longitude of each group is taken, and the group is referred to this point as the origin of co-ordinates; the mean co-tidal hour is also taken. In the equation of the co-tidal line

$$\mathrm{M}x + \mathrm{N}y = z,$$

x and y represent the known co-ordinates, the difference of longitude being reduced to the same unit of the geographical mile as the difference of latitude. The coefficients M and N are determined by the equations,

$$\mathrm{M}\,\Sigma x^2 + \mathrm{N}\,\Sigma xy = \Sigma xz$$
$$\mathrm{M}\,\Sigma xy + \mathrm{N}\,\Sigma y^2 = \Sigma yz.$$

$-\frac{\mathrm{N}}{\mathrm{M}}$ is the tangent of the angle which the co-tidal line makes with the meridian, and $\sqrt{\mathrm{M}^2 + \mathrm{N}^2}$ gives the motion of the tide-wave, or difference of co-tidal hour, for one geographical mile perpendicular to the co-tidal lines.

In this I have merely followed the admirable example given by Professor Lloyd, in the joint paper of himself and Colonel Sabine on the magnetic survey of the British Islands.

Having investigated separate groups in this way, it is easy to draw the co-tidal curves which represent the observations. The investigation tests the grouping in an interesting way, by the coincidence or discrepancy of the computed and observed co-tidal hours.

The first approximate indication from the discussion is the correspondence of the tides on our Atlantic coast to the supposition of a tide-wave moving from S. 53° E. to N. 53° W.; but this is only a rude statement of the phenomena.

In scanning the co-tidal hours along the coast, we observe that they divide themselves into two groups, one south of Martha's Vineyard, and the other north of Cape Cod, with a small anomalous group between them.

The co-tidal hours from Cape Florida to Quicks' Hole, Martha's Vineyard Sound, are not greater than $13^{h.}\ 30^{m.}$, nor less than $11^{h.}\ 38^{m.}$. Hatteras is decidedly, as was pointed out by the Master of Trinity, the Rev. Mr. Whewell, a point of divergence, the establishments on the north and east of it, and on the south and west of it, being, as a general rule, greater than the establishment of Hatteras. Our value for the co-tidal hour there differs from Mr. Whewell's, and further observations now in progress will decide between the two results.

From Provincetown, Cape Cod, to Cape Sable Island, the co-tidal hours vary between $12^{h.}\ 55^{m.}$ and $15^{h.}\ 43^{m.}$

Beginning at Fort Pulaski, Tybee Entrance, Savannah River, we have a good series of tidal stations to Hatteras, five in number, forming group *a*, Table No. III. The mean co-tidal hour is $11^{h.}\ 52^{m.}$, and the angle which the co-tidal line makes with the meridian is 59° 9′, agreeing very nearly with the trend of the coast.

The motion perpendicular to the direction of the co-tidal line is twenty-four miles in half an hour, agreeing nearly with the velocity due to the depth, as will be seen by inspecting the chart of co-tidal lines which accompanies this paper, upon which the fifty and one hundred fathom curves are drawn from the best data we yet have. The mean discrepancy of the computed establishments and of those observed for this group is $16^{m.}$, that of Beaufort differing most, and of Cape Fear the least.

If no correction had been applied for the positions of the tide-gauges within harbors, the results would have been as stated in group *a bis*, Table No. III., which, while the position of the co-tidal line is but little changed, gives a result for the movement of the tide-wave which is entirely too small, as must of course be the case.

The next group north and east of this, *b*, consists of Old Point Comfort, Delaware Breakwater, Cold Spring Inlet, and Sandy Hook, embracing a part of the coast having the same general direction. The mean co-tidal hour is 12h. 18m., the angle of the co-tidal line with the meridian is 22° 21′, agreeing again with the general trend of the coast, which is about 26°, the true value of the motion being masked by the irregularities of the establishments. The computed establishments agree well with the observed, differing but five minutes in the average, and six minutes at the greatest.

In the next group, *c*, four stations are placed from Sandy Hook to Quicks' Hole; namely, Sandy Hook, Point Judith, Newport carried to the entrance of the bay by the depth, and Quicks' Hole. The mean co-tidal hour is 12h. 6m., the direction of the co-tidal line 63° 27′ east of the meridian, and the distance gone over by the tide-wave in half an hour, forty miles. The mean difference of the computed and observed establishments is less than three minutes, and the greatest difference five minutes. Fire Island and Montauk are omitted from this series as anomalous. If, however, they are included, the result shown in *c bis,* Table No. III., is given, in which the direction of the co-tidal line is 58° 56′, and from which a decidedly erroneous velocity results.

Montauk looks like a point of convergence, as termed by Mr. Whewell, but its result is uncertain, and the observations there and at Fire Island must obviously be repeated.

The group *d* consists of six stations between Cape Cod and Cape Ann; namely, Provincetown, Wellfleet, Boston, Nahant, Salem, and Gloucester. It gives for the mean co-tidal hour, 15h. 15m.; for the inclination of the co-tidal line to the meridian, 31° 17′; and for the distance gone over by the tide-wave in 30m., 32 miles. The average difference between the computed and observed co-tidal hour is less than 4m., and the greatest difference less than 10m..

Group *e* contains Newburyport, Portsmouth, Portland, Fourchue and Ellenwood's Islands, and Cape Sable. The mean co-tidal hour is 14h. 37m.; the angle of the co-tidal line with the meridian, 44° 4′; and the motion of the tide-wave, 30 miles in 30m. The average difference of the computed and observed co-tidal hours is rather more than 10m., and the greatest difference in the case of Cape Sable

Island amounts to $23^{m.}$. Newburyport, which is brought to the sea establishment by the depth, differs but $6^{m.}$ in the computed and observed numbers.

The twelve stations of the last two groups combined in one group, give $14^{h.}$ $56^{m.}$ for the mean co-tidal hour; 38° 46′ for the inclination of the co-tidal line; and 33 miles in half an hour for the motion.

Between *c* and *d* is the anomalous group which occupies Martha's Vineyard and Nantucket Sounds, and includes the sea of the southern and eastern shores of these islands, and the passage between them. A side sketch shows the anomalous co-tidal hours varying from $12^{h.}$ $19^{m.}$ at Menamsha Bight, and $12^{h.}$ $38^{m.}$ at Wood's Hole, to $16^{h.}$ $2^{m.}$ at Holmes' Hole. The observations yet collected are not sufficient to trace with precision the details of these changes, though abundantly so to establish the general phenomena. They show conclusively that this is a case of interference, and point to the nature and amount of it. I prefer to obtain the further observations necessary to give the particulars of this interference before entering upon a discussion of this curious series. The heights concur with the times in giving the same solution to these cases, a mean rise and fall of one foot at Nantucket Island being placed as it were between a rise and fall of 3.3ft. at Menamsha Bight, and of 11.5ft. at Provincetown. The shoal ground off Nantucket, and its influence in the direction of the co-tidal lines, are roughly traced upon the chart.

Group *f* is at the extreme southern portion of the series, where the tide-wave is turned by the Bahama Banks, and makes its way through the Straits of Florida. The three stations in that group are Cape Florida, Cape Canaveral, and St. John's Entrance; of these Cape Canaveral affords but a vague result. The co-tidal line makes an angle of 117° 12′ with the meridian, and the motion in half an hour is 29 miles.

These results are collected in Table No. III., on next page.

TABLE NO. III.

Letters of Groups.	GROUP OF STATIONS.	Mean Co-tidal Hour. h. m.	$-\frac{N}{M}$	Angle with Meridian. ° ′	Incr. of Z. for every Min. of Dist. perp. to Co-tidal Line.	Geog. Miles perpendicular to Co-tidal Lines for 30m. of Z.	
a	Five stations from Savannah to Hatteras,	11 52	1.303	50 9	1.25	24	Tybee, Charleston, Cape Fear, Beaufort, and Hatteras.
a bis	The same uncorrected for depth,	12 16	1.298	52 23	2.31	13	
b	Four stations from Old Pt. Comfort to Sandy Hook,	12 18	0.411	22 21	4.47	6	Old Point Comfort, Delaware Breakwater, Cold Spring Inlet, Sandy Hook.
c	Four stations from Sandy Hook to Quicks' Hole,	12 6	2.00	63 27	0.76	40	Sandy Hook, Point Judith, Beaver Tail, Quicks' Hole.
c bis	Six stations from Sandy Hook to Quicks' Hole,	12 10	1.638	58 36	0.13	231	Sandy Hook, Fire Isl., Montauk Point, Pt. Judith, Beaver Tail, Quicks' Hole.
d	Six stations from Provincetown to Cape Ann,	15 15	0.608	31 17	0.924	32	Provincetown, Wellfleet, Boston, Nahant, Salem, Gloucester.
e	Six stations from Cape Ann to Cape Sable,	14 37	0.968	44 4	1.006	30	Newburyport, Portsmouth, Portland, Fourchue Isl., Ellenwood's Isl., Cape Sable.
e bis	Twelve stations fm Provincetown to Cape Sable,	14 56	0.803	38 46	0.910	33	
f	Three stations: — Cape Florida, C. Canaveral, and St John's,	12 48	—0.514	117 12	1.05	29	

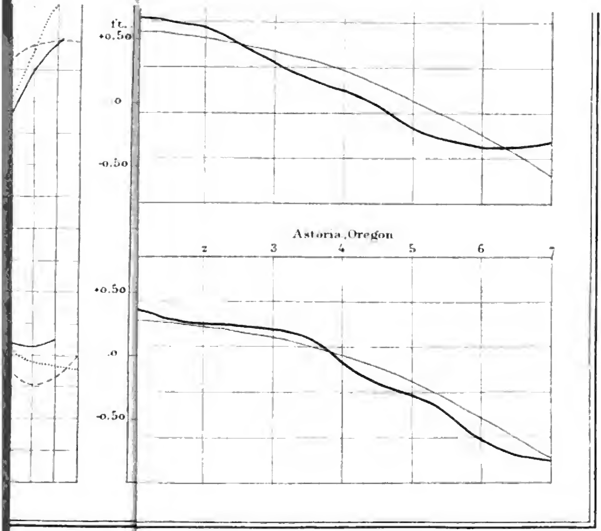
From the general indication of these groups, and with the approximate form of the curves of depth, it is not difficult to trace the probable forms of the co-tidal lines. With the assistance of Mr. Pourtales, the chart now presented has been prepared. It shows the co-tidal line of $12^{h.}$ following the trend of the Southern coast from Tybee towards Hatteras, running close to the shore as it passes southward into the Straits of Florida, interrupted between Cape Lookout and Hatteras in passing northward and eastward, appearing again north and east of Hatteras, following the coast inwards towards Sandy Hook and then towards Point Judith, and leaving the coast off the shoals of Nantucket and Cape Cod. The lines of $11\frac{1}{2}^{\mathrm{h.}}$ and $11^{\mathrm{h.}}$ are approximately drawn outside of the line of $12^{\mathrm{h.}}$, which, in general, is quite near to the coast, and conforms to its sinuosities from Tybee to Narragansett Entrance.

The co-tidal line of $15^{\mathrm{h.}}$ is the one which characterizes the indentation between Cape Cod and Cape Sable, the probable connection of the depth of the sea there and of the co-tidal lines being traced on the chart. The lines connecting these two systems are uncertain. The tide-wave appears to move backward from the northern extremity of Cape Cod to the southern, or Monomoy Point; but this part of the subject requires further observations for its elucidation.

I have tried, in discussing these observations, several other groups, but chiefly to learn where stations must be multiplied, or observations repeated, and it is not necessary to occupy the time of the Association with a detail of these trials, or with their results. They will be used in the progress of the further observations.

2. Comparison of the Diurnal Inequality of the Tides at San Diego, San Francisco, and Astoria, on the Pacific Coast of the United States, from Observations in Connection with the Coast Survey. By A. D. Bache, Superintendent. (Communicated by authority of the Treasury Department.)

At the meeting of the American Association in August, 1853, I submitted some remarks on the diurnal inequality of the tides, as observed at San Francisco. I propose now to compare this important inequality at the three ports of San Diego in California, San Francisco in California, and Astoria in Oregon. The results are the first fruits of the tidal observations under the immediate charge of Lieutenant Trowbridge, of the Corps of Engineers, to which I referred at the same meeting, as in progress. The series is intended to develop the tidal phenomena of that coast, and the three stations referred to are those for permanent reference, at which self-registering tide-gauges have been put up. The results now communicated are derived from observations at Astoria from July 11th to October 31st, 1853; at San


ft.
+0.50
.0
-0.50
Astoria, Oregon
2
3
4
5
6
7
+0.50
.0
-0.50

Francisco, from January 17th to February 15th, 1852, and from January 5th to February 26th, 1853; and at San Diego, from September 22d to November 31st, 1853. All the observations were made with Saxton's self-registering gauge. I submit specimens of the actual curve traced by the gauge. The results have been computed under the direction of L. F. Pourtales, Esq., Assistant United States Coast Survey, by Messrs. H. Heaton and P. R. Hawley, and the diagrams were drawn by Mr. C. Fendall.

A part of the curves of observation on a reduced scale is shown in diagram No. 1. The place of observation, and the dates for each series of curves, are stated on the diagram. The diagram represents the times from 0 hours to midnight, on a scale in which the distance between the vertical lines corresponds to two hours, and the heights on a scale of half a foot of rise or fall of the tide, to each division between the horizontal lines. The phases and declinations of the moon are inserted at the top of the diagrams, and the times of the moon's transit at the foot.

The curves have a striking similarity, and show a large diurnal inequality in both high and low water, in time and height at or near the greatest declination of the moon. The greatest inequality of the height is 2.76 ft. at Astoria, 2.40 ft. at San Francisco, and 2.77 ft. at San Diego. The mean rise and fall, estimating the highest high and lowest low water of each day only, is for the three places, respectively, 7.86 ft., 5.92 ft., and 5.46 ft. Some of the daily curves of San Diego near the period of greatest declination of the moon approach in form to those at Fort Morgan on the Gulf of Mexico.

The curves of half-monthly inequality of high and low water will be so much better determined hereafter, that I merely refer to them now, in passing, to show their general resemblance to those formerly produced for San Francisco.

The crude corrected establishment for the three places is, —

	h.	m.
For Astoria,	12	53
" San Francisco,	12	4
" San Diego,	9	37

The value of A (the tangent of the difference of luni-tidal interval for three and nine hours), and of E of Mr. Lubbock's notation (half the difference in height of neap and spring tides divided by 2 A), are for

	A		E
	In time.	In arc.	$\frac{h_1 - h_2}{2\,\mathrm{A}}$
	h. m.	° ′	
Astoria, ...	1 8	17 30	2.83
San Francisco, . .	1 18	19 30	1.59
San Diego, . . .	1 26	21 30	1.71

These of course are but approximations.

The following table shows the diurnal inequality in time and height of high and low water at the three places before named.

Diurnal Inequality in Intervals and Heights of High and Low Water at San Francisco, San Diego, and Astoria.

Inequality in Interval of High Water.			Inequality in Height of High Water.			Inequality in Interval of Low Water.			Inequality in Height of Low Water.		
San Francisco.	San Diego.	Astoria.	San Francisco.	San Diego.	Astoria.	San Francisco.	San Diego.	Astoria.	San Francisco.	San Diego.	Astoria.
h. m.	h. m.	h. m.	ft.	ft.	ft.	h. m.	h. m.	h. m.	ft.	ft.	ft.
1 44	1 56	1 19	—1.85	—1.62	—1.94	—0 51	—2 0	—1 15	3.01	2.90	3.81
1 57	1 16	2 0	—1.81	—1.18	—1.58	—1 3	—1 13	—1 12	3.44	2.87	3.27
1 47	1 36	1 14	—1.63	—0.67	—1.31	—0 47	—0 35	—0 53	3.75	2.48	3.20
2 17	1 12	1 5	—1.59	—0.17	—1.02	—0 40	—0 5	—0 41	3.72	1.76	2.56
1 41	0 46	0 53	—1.62	0.07	—0.75	—0 40	0 0	—0 33	3.46	1.01	1.68
1 43	0 17	0 33	—1.33	0.45	—0.17	—0 30	0 6	—0 16	3.07	0.28	1.25
1 41	—0 5	0 18	—1.05	0.83	0.27	—0 23	0 30	0 2	2.57	—0.35	0.74
1 20	—0 20	—0 2	—0.66	1.20	0.67	0 7	0 29	0 19	2.01	—0.99	0.09
0 39	—0 37	—0 16	—0.17	1.49	1.21	0 38	0 33	0 41	1.37	—1.39	—0.61
0 52	—0 58	—0 27	0.32	1.78	1.42	1 5	0 47	1 9	0.50	—1.70	—1.14
0 23	—1 16	—0 51	0.71	1.97	1.53	0 50	1 17	1 4	—0.41	—2.05	—1.74
—0 18	—1 15	—1 12	1.72	2.10	1.54	0 58	1 36	1 9	—1.44	—2.26	—2.70
—1 1	—1 50	—1 33	1.90	2.04	1.63	1 1	1 57	1 2	—2.19	—2.34	—2.75
—1 42	—2 23	—1 47	2.01	2.25	1.42	1 14	1 45	1 3	—2.85	—2.65	—3.36
—1 55	—2 51	—1 47	1.88	2.07	1.28	1 12	2 22	0 45	—3.36	—2.71	—3.39
—2 1	—3 10	—1 37	1.86	0.98	1.14	1 0	1 39	0 48	—3.48	—2.69	—3.27
—1 49	—3 3	—1 31	1.84	0.58	0.86	0 57	0 57	0 34	—3.48	—2.48	—2.98
—1 12	—2 0	—1 22	1.90	0.41	0.71	0 41	0 36	0 24	—3.28	—2.32	—2.33
—1 41	—1 26	—0 57	1.65	0.27	0.60	0 42	—0 4	0 21	—2.99	—1.81	—2.06
—1 26	—0 50	—0 33	1.42	0.09	0.20	0 35	—0 21	0 21	—2.50	—1.14	—1.40
—1 9	—0 46	—0 13	1.00	0.65	—0.25	0 20	0 31	0 6	—1.90	—0.93	—0.83
—0 59	—0 20	0 2	0.29	0.32	—0.30	—0 8	0 3	—0 21	—1.29	—0.29	—0.11
—0 32	0 5	0 17	—0.30	—1.08	—1.12	—0 35	—0 46	—0 35	—0.50	0.97	—0.81
—0 9	0 41	0 22	—0.97	—2.33	—1.70	—1 15	—0 38	—0 52	0.26	2.49	1.24
0 29	0 55	0 46	—1.42	—2.68	—2.00	—1 17	—1 16	—0 54	0.95	2.96	2.12
0 46	1 6	1 1	—1.56	—2.41	—2.18	—1 12	—1 42	—1 18	1.85	2.47	2.75
1 51	1 53	1 15	—1.70	—2.22	—2.25	—0 46	—1 43	—1 21	2.51	2.84	3.24
1 58	2 23	1 38	—1.62	—1.95	—2.01	—0 30	—2 0	—1 20	2.99	2.79	3.85
2 11	2 55	1 34	—1.35	—1 32	—2.54		—2 27	—1 8		3.07	3.79
Mean, 1 22	1 25	0 59	1.36	1.28	1.23	0 47	1 2	0 46	2.33	1.97	2.17

The whole difference in height and interval between the A. M. and P. M. tides is taken as representing the diurnal inequality, and no correction is made for the half-monthly inequality in preparing the tables. I may observe that, after trying the corrections with the half-monthly inequality as at present determined, no special advantage resulted; and until the half-monthly inequality is determined by more numerous observations, I adhere to this form of discussion.

The results are shown in diagram No. 2, in which the ordinates denote the inequality in interval and height on the days from zero of declination of the moon denoted by the abscissæ. The dots show the actual observations, and the curves are drawn with a free hand among them. The results for the three places are distinguished as marked on the diagram.

The following are the inferences from the discussion : —

1. In every case the inequalities increase and decrease with the moon's declination, reaching zero at or near the time of the moon's crossing the equator. The average epoch of the inequalities agrees almost exactly with the time of the zero of declination.

2. The inequalities in height of high water and in interval of low water increase and decrease together, and so for the inequality in time of high water and in height of low water, as was remarked in the case of San Francisco.

3. The declination of the moon and the inequality in interval of high water and in height of low water have contrary signs * at all three of the places, the reverse being true of the other two inequalities.

4. The inequality in the height of low water is in general greater than that of high water, as was before stated for San Francisco, the proportion of the average and maximum inequality is nearly as follows : —

	Average Inequality.	Maximum Inequality.
San Francisco, . .	1.8 to 1	1.9 to 1
San Diego, . . .	1.5 to 1	1.2 to 1
Astoria, ...	1.8 to 1	1.5 to 1

---

* The reverse is stated by mistake in my former paper.

5. The inequality in the interval of high water is in general greater than that of low water, as follows : —

	Average Value.	Maximum Value.
San Francisco, . .	1.7 to 1	1.8 to 1
San Diego, . . .	1.4 to 1	1.3 to 1
Astoria, ...	1.3 to 1	1.5 to 1

6. The average and greatest inequalities in interval and height are shown in the following table : —

Average Inequality.				Greatest Inequality.			
Time.		Height.		Time.		Height.	
H. W.	L. W.	H. W.	L. W.	H. W.	L. W.	H. W.	L. W.
h. m.	h. m.	ft.	ft.	h. m.	h. m.	ft.	ft.
1 22	0 47	1.36	2.33	2 17	1 17	2.01	3.75
1 25	1 2	1.28	1.97	3 10	2 27	2.68	3.07
0 59	0 46	1.23	2.17	2 0	1 21	2.54	3.85

7. The comparison of the values of the diurnal inequality in height with the theoretical expression M sin $2\,\delta'$, is given in the annexed table and diagram, in which the value of M is taken as 2.35. The inequality results are grouped by the declinations.

Comparison of Daily Inequality with Moon's Declination.

	San Diego.			Astoria.		
	Observed Inequality. H. to H.W.	M sin $2\delta'$.	Diff.	Observed Inequality. H. to H.W.	M sin $2\delta'$.	Diff.
	ft.	ft.		ft.	ft.	
1	2.03	1.90	.13	1.94	1.80	.14
2	1.93	1.83	.10	1.75	1.72	.03
3	1.55	1.70	—.15	1.73	1.60	.13
4	1.26	1.48	—.22	1.32	1.40	—.08
5	.84	1.14	—.30	.98	1.10	—.12
6	.63	.75	—.12	.49	.71	—.22
7	.69	.28	.41	.24	.28	—.04

The observations of which these form a part are still in progress, under the direction of Lieutenant Trowbridge, whose assiduity and intelligence have already been rewarded by the success which I had ventured to anticipate in my former reference to the tides of the Western Coast.

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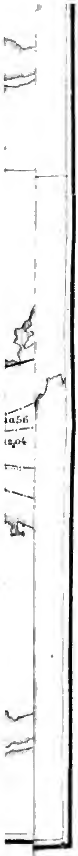


3. RESULT OF AN INVESTIGATION OF THE TIDAL CURRENTS IN LONG ISLAND SOUND, WITH AN APPENDIX CONTAINING TABLES, AND TWO SKETCHES. By CHARLES A. SCHOTT, of Washington City, D. C.

[The substance of this paper was communicated in a report to the Superintendent of the Coast Survey, in April, 1853, and is now published, with the permission of the Treasury Department.]

THE study of the current phenomena, as useful to the mariner as it is interesting to the scientific inquirer, becomes of additional interest in this particular locality, from the fact that the Sound is accessible to the ocean current, both at its eastern and western entrances, and from advantages that may result to navigation in this much frequented region from a more complete understanding of the motion of its waters.

In order to fully understand the currents, it is essential to consider the horizontal as well as the vertical motion of any particle; or the tidal currents proper, or ebb and flood stream, and the tides, or rise and fall. These are the horizontal and vertical components. To distinguish currents running always nearly in the same direction from those that reverse their motion, governed by the effects of the lunar and solar attraction, the latter have received the name of Tidal Currents; and such is the nature of those in Long Island Sound, though an almost imperceptible current setting towards the sea, the discharge of the river water, can barely be traced.

The known rise and fall of the tides will furnish the navigator with the means of adapting his soundings to the level of reference of his chart, generally the mean low water; and the known set and drift will guide him in his course.

The great tidal wave from the Atlantic enters the Sound between Point Judith and Montauk Point, divided into two branches by the intermediate position of Block Island; and another portion of this wave enters New York Bay, and, passing through Hellgate, meets the wave propagated through the Sound from the eastward.

It was a matter of importance and interest to know the place of meeting and the limits of variation of these opposite waves. The material for this investigation was furnished from the observations of currents and tides by the officers of the Survey, under instructions from the Superintendent, the former commenced in 1844 and continued to 1848, the latter dating further back. The range of the tide, or the mean vertical motion, the epoch of the tide as shown by the co-tidal lines and the direction of the flood or ebb stream, together with certain lines depending on the epoch of the horizontal motion, presently to be explained, furnish us with as many separate means for determining the locality of the meeting of the eastern and western wave. Under these different points of view, we can arrive with a very close approximation at the motion of the water throughout the Sound.

Considering, first, the mean or average range, we observe that, in a regular straight or slightly curved channel of nearly the same profile, and accessible from both sides, opposite waves will produce the greatest range where they meet, and equal to the sum of the ranges at the entrances, and in cases where the channel towards the head is wedge-shaped, this range will still further be increased proportional to the contraction of the profile. At Montauk the mean range is 2.0 feet, and at Sandy Hook 4.8 feet; and from the narrowing of the Sound the greatest range observed is 1 foot more than the sum, or 7.8 feet. From a table of observations of ranges at nineteen stations a chart has been constructed exhibiting the lines of equal mean range for each half-foot of change. The line of the maximum range by this chart is off Sand's Point, and the most rapid increase occurs in the passage through Hellgate.

Following, next, the co-tidal lines constructed from a table of mean luni-tidal intervals observed at twenty-three stations, and following up the channel ways from both entrances, the crossing of the lines of the same hour will indicate the place of meeting of the opposite waves, or the point where the high water takes place latest in time. To the eastward or westward of it high water occurs sooner. The co-tidal lines were constructed for local times, answering the purpose, when the difference in time for the extent of the chart is nearly 12 minutes. Owing to the want of a sufficient number of observations, the co-tidal lines for the western portion of the chart could not be traced, and the 7½ hour and 8 hour lines near Block Island are uncertain; there is, however, material enough to point out Hewlett's Point as the position of the latest tides, with the hour $\text{XI}^{\text{h.}}$ $\text{XXII}^{\text{m.}}$. At Montauk we had $\text{VIII}^{\text{h.}}$ $\text{X}^{\text{m.}}$, and at Sandy Hook $\text{VII}^{\text{h.}}$ $\text{XXXI}^{\text{m.}}$. It must be observed that the observations cannot be considered as closed, and that a further accumulation of data will enable us from more complete charts to follow and predict the currents and tides with much more precision than is at present possible.

The actual observed direction of the flood and ebb stream, or the direction of the current immediately following or preceding the low or high water, however, will best point out the head-waters of the Sound. To the eastward and westward of Throg's Neck, the currents have thus been found to flow in opposite directions.

From the above we conclude that the tides meet near Hewlett's Point as the mean position, and that the variable limits are Sand's Point and Throg's Neck, a few miles to the eastward and westward of it. The semi-mensual and diurnal inequalities, together with the action of the wind, produce these changes.

The currents proper may be considered under the three following heads: — The direction and velocity, or set and drift, and the time at any stage of the stream.

A chart was constructed exhibiting the lines of direction for the ebb or outgoing stream when at its greatest velocity. This chart also was found to answer for the flood or ingoing stream, the set differing nearly 180°. These curves of direction are perhaps the most useful to the mariner. They follow the channel, or are nearly parallel to the shores. They were drawn equidistant as far as the observations and the locality would admit of, and the velocity in miles per hour was written in figures against the places of observation. If the observations had been more numerous, the better method of varying the distance of the lines inversely proportional to the velocity, according to a definite scale, would have been preferable. The latter representation, proposed by Professor Bache, was adopted for other charts of the Survey.

Following any of the curves of direction, the change in velocity may be seen by inspection. It is not, however, to be supposed that a floating body would be carried throughout the length of any of the lines, as the reverse motion, after about six hours' duration, would restore it to nearly its original position. It was shown in an accompanying table, that the velocity of the flood stream was a little less than that of the ebb, caused by the discharge of the river water in the Sound. The more remarkable velocities were found as follows: 4.7 knots in the

Race (opposite the west end of Fisher Island); 5.9 knots in Hellgate; 3.7 knots on an average in New York Harbor, and 3.0 in the Bay; 2.2 knots is the average drift in Long Island Sound. The drift is a function of the size of the section of the channel and the quantity of water to be discharged through it.

The total number of current stations used in the discussion, and extending over the Sound and New York Bay and Harbor, was seventy-five, occupied generally for thirteen hours each.

The time of turn, or the epoch of the current, occurring shortly after high water, is the last datum required to be known in order to predict the currents as we now do the tides. This I have attempted to ascertain by taking the intervals of time of the moon's superior or inferior transit, and the time of the middle of slack water at or immediately following the high-water stand. It must be observed that it is by no means uncommon to perceive horizontal motion during the time of stand, or vertical motion during the time of slack water. The interval just spoken of I call luni-current interval, analogous to luni-tidal interval, and the mean of these luni-current intervals gives the mean establishment of the tidal currents; thus, by uniting all the places of the same current establishment by lines, co-current lines will be obtained. These lines may be regarded as corresponding to the co-tidal lines. The epoch of the beginning of the ebb stream for these curves was chosen for the purpose of a more ready comparison with the co-tidal lines, the difference indicating how much sooner or later one or the other of the motions commences, or the difference in time between the middle of stand and the middle of slack water immediately preceding the ebb stream. The duration of the slack water both for the ebb and flood streams, and their duration, were given in tables, so that the condition of the current at any given time may easily be inferred; the process being precisely similar to that employed for predicting tides. No current stations have as yet been occupied during the whole of a lunation; but to arrive at any of the inequalities of the tidal currents in this way appears to me less preferable. The corrections may safely be taken as found from the inequalities of the tides. These co-current lines disclose some remarkable features. First, their general tendency to run parallel to the channel or shore, arising from the more rapid propagation of the wave in deep water; secondly, the motion of the water after the high-water stand commences, first near the shore, and last in the channel way. The current establishments have been given in a tabular form, from which the co-current lines have been constructed. The instant of the beginning of the ebb stream (mean of slack water) was chosen for the epoch. It was not deemed necessary to refer the times to one meridian. Taking means of these luni-current intervals of stations near mid-channel, I found the hour,

	h.	m.
Between Block Island and the Race, from 8 stations,	XI.	XXXIII.
In the middle of the Sound (widest part), from 7 stations, ...	XII.	
Near the head of the Sound, from 10 stations, .	XII.	XXXVIII.

The number of observations at each station ranges from one to three, taken in different years.

	h.	m.
In New York Bay we have, from 13 stations, .	VIII.	XX.
In New York Harbor, from 3 stations, . .	IX.	XXIX.
In Hellgate, from 10 stations, ...	IX.	XXXIII.
Between Hellgate and Throg's Neck, from 5 stations,	X.	XLI.

Showing a remarkably slow propagation between Hellgate and Hewlett's Point, which is accounted for below. The mean uncertainty in the above figures was shown in a table, and was found not greater than the irregularities of the tides from an *a priori* consideration would indicate.

The ebb stream at the eastern entrance commences three hours later than at the western entrance, although the tides differ but one hour at Montauk and Sandy Hook. At half ebb tide the water in the channel off Montauk Point begins to run out, and before two thirds of the ebb tide is completed, the water in the channel at the head of the Sound begins its outward motion. The phenomena is similar in New York Bay, where the ebb stream begins its motion at one sixth of the ebb tide, but at the head east of Hellgate, the ebb stream is not perceptible till the tide at the entrance of the Bay is almost reversed, or at five sixths of the ebb tide.

Lines showing the level of the surface of the Sound at any stage of the tides may be constructed with the aid of the co-tidal lines and the curves of mean equal range. As already indicated, the water begins its motion first nearest the shore, and not until a considerable time has elapsed in the channel. The X., XI., and XII. hour curves of the co-current lines near the Race, on both sides of the Sound, are in this respect particularly interesting. The knowledge of this is made available by vessels navigating this part of the Sound, and the earlier or later setting in of the currents is known to the local pilots. In mid-channel throughout the Sound the outward motion commences shortly after the high water at the head of the Sound, and evidently depends upon it, though it cannot be recognized west of Throg's Neck, on account of the disturbing influence of the rocky and narrow passage through Hellgate. The following is a practical note deduced from the observations. The currents commence running along the northern and southern shore of the eastern part of the Sound at various intervals from one to two hours before running in the channel. The currents along the shore between New Haven and New London are earlier by one hour than between Fisher's Island and Point Judith, making in all a difference of two hours and a quarter between the first-named places, and of one and a quarter between the last-named and the beginning of the motion in the channel. The currents through the entrances formed by Block Island begin to run simultaneously, and about one hour earlier than in the Race. The ebb through Plum Gut lasts about two hours longer, and the flood so much shorter, than at other places in the vicinity.

To find the time of slack water at the beginning of the flood, add the duration of the ebb stream (which was given in a table) to the luni-current interval. The duration of the ebb stream at the entrance, middle, or head of the Sound appears to be the same, and this holds good for the duration of the slack water.

All the remarks and deductions refer to the surface current. The motion in any given vertical section appears to begin first near the surface, but shortly after the velocity is nearly the same throughout the whole depth. There appears to be no difference in the direction of the stream at or below the surface.

I had proceeded thus far in the investigation when Professor Bache suggested to me the attempt to ascertain how far the circumstances of propagation of the current could be explained by the mean depth of the Sound. This I have attempted in the following investigation, which greatly facilitates the construction of the time curves, and thus aids in the prediction of the tides for places where no direct observations have been made.

The tidal wave, Russel's great primary wave and Airy's free tide wave, a wave of the first order, or any wave of the second order that has, by approaching the shore, become a wave of translation, travels with a velocity equal to that acquired by a heavy body falling freely by gravity through a height equal to half the depth of the fluid, reckoned from the top of the wave to the bottom of the channel. In any other than a rectangular channel the effective height is from the top of the wave to the centre of gravity of the section. In a primary wave there is no oscillation proper, but the particles of the fluid in a vertical section by the transit of the wave are raised, moved laterally, and permanently brought to rest in the direction of the motion in a new place with the same extent of transference of each particle throughout the whole depth of the wave. The height of this wave increases in a channel of the form of a wedge and of equal depth nearly in the inverse ratio of the square root of the breadth.

Waves of the second order, such as those caused by the wind, oscillate, either circularly or elliptically, and their velocity is not a function of the depth of the water.

To apply the above empirical laws it was necessary to obtain the correct time of the transit of a wave in the channel over two places at a considerable distance apart. A position near the Race, and another off Sand's Point, were found admirably fitted for the purpose, and the time of transit as observed on the shore had only to be referred to the mid-channel.

Let h denote the effective depth, and v the corresponding velocity, then we have the simple formulæ for feet and seconds of time,

$$h = 0.015536\ v^2$$
$$v = 8.0227\ \sqrt{h},$$

or for h in feet and v in statute miles per hour $= [0.73800]\ \sqrt{h}$, the number within brackets being a logarithm. Subtracting 1 minute for the time of transmission of the wave from the channel to the place of observation at Sand's Point (computed by the above formula), and 7 minutes for New Rochelle, from the observed time of the transit of the crest of the wave or high water at this place, we find for the transit in the channel $\text{XI}^{\text{h.}}\ \text{XII}^{\text{m.}}$, and similarly $\text{IX}^{\text{h.}}\ \text{XXI}^{\text{m.}}$ for the same at the Race. This difference increased by $10^{\text{m.}}$ for difference in longitude, leaves $2^{\text{h.}}\ 1^{\text{m.}}$ as the observed time of the transmission of the wave from the Race to Sand's Point, a distance of ninety-five miles.

To obtain the computed time, the space was subdivided to allow mean depths to be used, and by adding the times together for the several sections as computed by the above formula, an interval of $2^{h.}\ 19^{m.}$ was found, differing but $18^{m.}$ from the observed time.

An analytical investigation by Andrew John Robertson (see London, Edinburgh, and Dublin Philosophical Magazine, February, 1853), based upon Scott Russel's experiments, and with consideration of Mr. Earnshaw's paper (Philosophical Magazine, December, 1850 – 51), gives for the velocity of the transmission of the wave of translation $v = (h + 2k) \sqrt{\frac{g}{h+k}}$; and on another supposition, simplifying the investigation, $v = (h + 2k) \sqrt{\frac{g}{h}}$, where k = depth of undisturbed waters, $2k$ = height of a positive wave, g = accelerating force of gravity. Expressing h in feet, and v in miles per hour, we have $v = [0.58739]\ (h + 2k) \sqrt{\frac{1}{h}}$, the number inclosed in brackets being a logarithm. Applying this formula, the time for traversing the ninety-five miles becomes $2^{h.}\ 14^{m.}$, being 5 minutes nearer the observed time, and differing but a few minutes from it.

The wave, travelling at the rate of sixty miles an hour near the Race, arrives at the head of the Bay with but a little more than half that velocity, sending out branches towards the shores; hence the parallelism of the co-tidal lines with the shore line. The propagation of the wave from some point in the channel towards the shore station takes place in the shortest time possible, and the line of propagation can be found by the method of trial and error with tolerable accuracy. The above has been verified by our tidal observations on the shores of the Sound; but there is yet some obscurity in the time of the tides near the eastern entrance, which will be cleared by future observations.

Referring some observed tides below and above the Race to the channel, the resulting retardation of the tidal wave by the contraction of the shore lines at this place I computed to 8 minutes. The explanation for the remarkably slow transmission of the wave east of Hellgate will be found in the intricacies of the channel; these narrow passages, turning frequently round acute angles, and destroying the acquired momentum by friction, will not permit us to calculate the time of transmission through them.

At the Southwest Ledge the sea is said to break in heavy weather; the depth, however, being six fathoms, would, for a perfect breaker, require a wave of seventy-two feet from top to bottom. The interference of waves appears to favor the formation of imperfect breakers.

The currents on the dangerous ground of the Nantucket Shoals are now undergoing investigation, the result of which will have a more practical bearing to the navigator, and may be communicated at a future time.

APPENDIX.

No. I.— *Table of the Observed Mean Range of Tides.*

Eastern Series.	Feet.	Western Series.	Feet.
Montauk Point,	2.0	Sandy Hook,	4.8
Point Judith,	3.1	Governor's Island,	4.9
Beavertail Light,	4.0	Spitfire Pot Cove,	6.3
Stonington,	2.7	North Brother,	7.2
New London,	2.6		
Sachem's Head,	5.3		
New Haven,	5.8		
Bridgport,	6.6		
Sheffield Island,	7.4		
Huntington Bay,	7.6		
Great Captain's Island,	7.5		
Oyster Bay,	7.2		
Sand's Point,	7.7		
New Rochelle,	7.6		
Throg's Neck,	7.2		

No. II.— *Table of the Mean Establishment of Tides.*

Eastern Series.	h.	m.	Western Series.	h.	m.
Montauk Point,	VIII.	10	Sandy Hook,	VII.	31
Point Judith,	VII.	32	Governor's Island,	VIII.	31
Beavertail Light,	VII.	56	Hallett's Cove,	X.	0
Quick's Hole,	VII.	32	Spitfire Pot Cove,	X.	48
Watchhill,	IX.	6	North Brother,	XI.	16
Stonington,	IX.	7			
Little Gull Island,	IX.	38			
New London,	IX.	33			
Sachem's Head,	XI.	0			
New Haven,	XI.	16			
Bridgport,	XI.	11			
Sheffield Island,	X.	58			
Huntington Bay,	X.	51			
Great Captain's Island,	XI.	1			
Oyster Bay,	XI.	7			
Sand's Point,	XI.	13			
New Rochelle,	XI.	22			
Throg's Neck,	XI.	20			

No. III.—*Table showing the Set and Greatest Drift for the Ebb and Flood Stream.*

The greatest velocity of the current takes place nearly midway, between two consecutive slack waters. The numbers of the stations refer to the published charts.

EASTERN SERIES.

No.	Ebb Stream.		Flood Stream.	
	Greatest Velocity.	Direction.	Greatest Velocity.	Direction.
	Miles per hour.	°	Miles per hour.	°
45	2.1	S. 22 E.	2.1	N. 11 W.
44	2.4	S. 43 E.	1.3	N. 53 W.
43	2.8	N. 68 E.	1.6	N. 17 W.
42	1.1	S. 61 E.	1.2	S. 82 W.
41	1.9	S. 50 E.	2.0	N. 51 W.
40	1.9	N. 86 E.	1.4	N. 84 W.
39	1.0	N. 59 E.	0.8	S. 82 W.
38	1.3	N. 58 E.	1.0	S. 78 W.
37	2.0	N. 84 E.	2.0	N. 81 W.
36	4.5	S. 56 E.	4.7	N. 78 W.
35	1.9	S. 77 E.	1.6	N. 55 W.
34	2.8	S. 67 E.	2.1	N. 53 W.
33	2.7	N. 70 E.	2.6	S. 78 W.
32	2.5	S. 71 E.	1.8	W.
31	1.7	S. 80 E.	1.8	S. 86 W.
30	2.0	N. 52 E.	1.4	S. 53 W.
29	1.5	S. 81 E.	1.5	N. 78 W.
28	1.6	S. 83 E.	1.4	N. 76 W.
27	1.5	S. 67 E.	1.2	W.
26	2.0	S. 76 E.	1.9	N. 84 W.
25	0.8	N. 70 E.	0.6	S. 71 W.
24	1.5	N. 76 E.	1.5	N. 63 W.
23	2.1	N. 64 E.	1.8	S. 80 W.
22	1.8	N. 88 E.	2.2	N. 84 W.
21	2.2	S. 84 E.	2.1	S. 71 W.
20	2.2	N. 77 E.	1.9	S. 81 W.
19	1.3	N. 73 E.	1.2	S. 62 W.
18	1.5	N. 58 E.	1.4	S. 33 W.
17	0.8	N. 62 E.	1.0	S. 64 W.
16	1.2	N. 68 E.	1.2	S. 76 W.
15	1.1	N. 75 E.	1.3	S. 46 W.
14	0.6	N. 50 E.	0.6	S. 42 W.
13	0.7	N. 35 E.	0.6	S. 58 W.
12	0.9	N. 34 E.	0.6	S. 25 W.
11	1.0	N. 14 E.	0.5	S. 26 W.
10	0.9	N. 45 E.	0.5	S. 15 W.
9	1.0	N. 28 E.	0.7	S. 25 W.

WESTERN SERIES.

Greatest Velocity observed at the Stations between Throg's Neck and Governor's Island.

No.	Ebb.	Flood.
	Miles per hour.	
7	1.1	1.5
6	1.0	1.3
5	1.0	0.5
4	1.0	0.6
3	1.0	1.4
1	2.7	2.9
1	4.6	5.9
2	2.7	4.2
3	2.8	2.4
4	2.6	1.2
5	4.0	2.2
6	1.0	0.7
7	2.2	3.9
8	4.7	4.2
9	3.7	3.2

Greatest Velocity and Corresponding Direction in New York Bay and Harbor.

Published Chart No.	Ebb.		Flood.	
	Greatest Velocity.	Direction.	Greatest Velocity.	Direction.
1	1.0	S. 47° E.	0.9	N. 32° W.
2	1.5	S. 50 E.	0.5	N. 36 W.
3′	2.5	S. 73 E.	1.3	N. 61 W.
3′	2.5	S. 49 E.	1.5	N. 49 W.
4	1.5	N. 51 E.	1.7	N. 76 W.
5	1.0	S. 66 E.	1.2	S. 87 W.
1	2.9	N. 74 E.	2.0	N. 71 W.
2	2.0	S. 51 E.	1.8	N. 62 W.
3	2.2	S. 71 E.	1.6	N. 8 W.
4	1.8	S. 67 E.	1.8	S. 78 W.
5	1.4	S. 53 E.	2.0	S. 31 W.
6	2.1	S. 60 E.	1.7	N. 55 W.
7	2.4	S. 34 E.	1.8	N. 52 W.
8	2.5	S. 15 E.	1.4	N. 26 W.
9	3.0	S. 44 W.	1.8	N. 47 E.
10	3.0	S. 47 W.	2.0	N. 47 E.
11	2.4	S. 30 W.	1.8	N. 20 E.
12	3.7	S. 56 W.	3.0	N. 68 E.
1	2.4	S. 25 E.	2.0	N. 20 W.
2	2.3	S. 58 E.	1.8	N. 58 W.
3	2.8	S. 75 E.	1.5	N. 20 W.

No. IV. — *Luni-current Intervals for the Middle of Slack Water after High-Water Stand, or for the Beginning of the Ebb or Outgoing Stream.*

EASTERN PART OF THE SOUND. 1846-7.

Station No.	Mean Luni-current Interval.	Number of Observations.	Luni-current Interval of Stations in or near the Channel.	Mean.
	h. m.		h. m.	
45	11 38	3	11 38	h. m. 11 33 Eastern Entrance of the Sound.
44	11 16	2	11 16	
43	11 42	2	11 42	
42	11 16	2	11 16	
41	9 59	3		
40	11 14	3	11 14	
39	9 53	3		
38	10 17	2		
37	10 0	2		
36	12 20	2	12 20	
35	11 2	3		
34	11 45	2	11 45	
33	11 15	2	11 15	
32	10 4	1		
31	10 3	3		
30	12 0	2	12 0	h. m. 12 0 Middle part of the Sound.
29	10 11	2		
28	12 9	2	12 9	
27	11 5	3		
26	11 22	3		
25	11 52	2	11 52	
24	10 58	2		
23	12 4	3	12 4	
22	10 56	1	10 56	
21	12 30	3	12 30	
20	12 20	3	12 20	
19	12 47	3	12 47	h. m. 12 38 Western part of the Sound.
18	12 55	2	12 55	
17	12 49	3	12 49	
16	11 54	2	11 54	
15	12 0	3	12 0	
14	11 48	2	11 48	
13	13 48	2	13 48	
12	12 44	3	12 44	
11	12 42	3	12 42	
10	12 46	3	12 46	
9	8 0*	3		

* Rejected.

WESTERN PART.

New York Bay and Harbor. 1844.

STATION.	NEW YORK BAY. Luni-current Interval.	CHANNEL. Luni-current Interval.	Mean.	NEW YORK HARBOR. Luni-current Interval.	Mean.
	h. m.	h. m.	h. m.	h. m.	h. m.
1	7 40	7 40	8 20		
2	7 56	7 56			
3	6 51 ? *				
4	8 50	8 50			
5	7 58	7 58			
6	6 39 ? *				
7	8 30	8 30			
8	8 15	8 15			
9	7 55	7 55			
10	8 19	8 19			
11	9 3	9 3			
12	9 18	9 18			
13	9 18				
14	8 16				
15	9 10			9 10	9 29
16	9 38			9 38	
17	9 38			9 38	
18	10 0				
19	8 39	8 39			
20	7 53	7 53			
21	8 3	8 3			

* Rejected.

STATION.	HELLGATE, 1845. Luni-current Interval.	Mean.	STATION.	BETWEEN HELLGATE AND THROG'S NECK. Luni-current Interval.	Mean.
	h. m.	h. m.		h. m.	h. m.
1	10 0	9 33	1	10 28	10 41
2	10 18		2	10 50	
3	9 36		3	11 8	
4	9 26		4	10 35	
5	9 28		5	10 25	
6	9 38				
7	9 15				
8	9 7				
9	9 9				
1 in 1846	9 31				

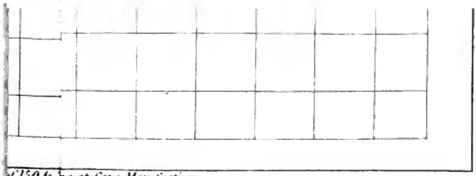
No. V. — *Showing the Irregularity in the Luni-current Intervals on Successive Tides in Long Island Sound.* 1846 – 47.

Station No.	Luni-current Intervals.			Mean.
	h. m.	h. m.	h. m.	h. m.
45	11 20	10 53	12 42	11 38
41	10 36	9 52	9 28	9 59
40	10 45	11 6	11 51	11 14
39	9 29	9 28	10 41	9 53
35	10 57	11 8	10 59	11 2
31	10 18	10 28	9 22	10 3
29	10 1	10 13	10 25	10 11
27	11 0	11 10	11 5	11 5
26	10 27	12 40	11 0	11 22
23	12 2	11 39	12 30	12 4
21	11 34	12 31	13 23	12 30
20	11 9	12 16	13 34	12 20
17	12 26	12 59	13 2	12 49
15	12 56	12 59	12 6	12 0
12	13 17	12 1	12 55	12 44
11	12 13	12 44	13 8	12 42
10	12 27	12 41	13 10	12 46
7	9 43	9 52	11 40	10 25
6	10 24	10 18	11 2	10 35
3	10 1	10 28	10 55	10 28
1	9 10	9 28	9 55	9 31

4. On the Distribution of Temperature in and near the Gulf Stream, off the Coast of the United States, from Observations made in the Coast Survey. By A. D. Bache, Superintendent. (Communicated by authority of the Treasury Department.)

I propose to present to the Association a brief summary of the result of observations made in the progress of the Coast Survey, in exploring the Gulf Stream, so far as the distribution of temperature is developed by them. The entire observations have been reduced anew under my immediate direction by Professor Pendleton, United States Navy, Assistant in the Coast Survey, who has also gone over with me, systematically, the discussion of the work preparatory to its publication in detail, and whose care, assiduity, and intelligence in the matter I desire here to acknowledge.

At the Cambridge meeting of the Association in 1849, I explained



f 150 & ns at Cape May Section
300 & 46 · Cape Henry Section 3 years
· Sandy Hook "
· St. Simons "
· Cape Fear "
· Charleston "

the general plan of the exploration of the Gulf Stream, and presented the results of the observations made up to that time by Lieutenants C. H. Davis, George M. Bache, S. P. Lee, and Richard Bache, of the United States Navy, in command of hydrographic parties in the Survey. Since then the work has been continued by Lieutenants T. A. M. Craven and J. N. Maffitt, United States Navy, and has been extended south from Hatteras to Cape Canaveral. In addition to the sections across the stream, upon which the temperature had then been examined between Cape Cod and Cape Hatteras, others have been since explored from Cape Fear, Charleston, St. Simon's, St. Augustine, and Cape Canaveral, and new and interesting results have been developed in relation to the distribution of temperature across the sections, and to the connection, at least in some of them, between the peculiar distribution and the form of the bottom of the sea.

The examination now made extends from about 42° N. Lat. to 28½°, and from about 65½° W. Long. to 80½°, and authorizes the construction of a chart of the Gulf Stream, showing the distribution of temperature in and near it, not only at the surface, but at various depths.

1. *Distribution of Temperature at Different Depths.*

Having gone very fully into this subject, which was one of the first satisfactorily shown by the observations, I do not intend to repeat here what was then stated, except in a general way. The distribution of temperature, below a certain depth, in the cold current which exists between the shore and the Gulf Stream, and over which the warm waters of the Gulf flow, thinning out as they approach the land, was shown to belong to a state of equilibrium of temperature which would be assumed, by a mass of water having warm water above it and cold water below it, to be represented by a logarithmic curve, and therefore to be due to conduction. That in the Gulf Stream varied according to a different law, indicating a disturbance of equilibrium.

Diagram No. 1 shows the distribution of temperature with depth in the water between the shore and Gulf Stream, as deduced from the observations of Lieutenant G. M. Bache. The ordinates of the curve represent the depths, and the abscissæ the temperatures. The depths in fathoms are written at the side of the diagram, and the temperatures, by Fahrenheit's scale, at the top. The positions at which the temperatures at various depths, recorded in this diagram, were obtained, was in Lat. 36° 15′ N., Long. 73° 52′ W., on the section intended to be made from Cape Henry perpendicular to the axis of the stream. This curve and others of the same kind were compared with the logarithmic curves which would best represent the observations, and their close coincidence with them shown.

The curves were deduced by least squares from an ingenious investigation by J. H. Lane, Esq., then of the Coast Survey, now one of the chief examiners in the United States Patent-Office.

Diagram No. 2, taken also from Lieutenant G. M. Bache's observations, shows the character of the curve of distribution near the axis of the Gulf Stream. These particular results were obtained in one of the positions on the Cape Henry section in Lat. 35° 53′ N. and Long. 73° 34′ W. The projecting form of the curve towards 300 fathoms, and the moderate change of temperature, ten degrees, from 150 to 400 fathoms, shown in that diagram, are characteristic features of the distribution in similar positions. The change from the surface to 150 fathoms was 17° Fahrenheit.

Diagram No. 3 represents a corresponding curve to No. 2, from Lieutenant Maffitt's observations on the Charleston section, in Lat. 33° 58′ N., Long. 73° 58′ W. In this the change between 100 and 400 fathoms is still less than in the former case, being but five degrees.

Diagram No. 3 *bis* shows the curve corresponding to that of diagram No. 1, but on the Charleston section, nearer to the shore than the axis of the stream, in Lat. 31° 48′ N., Long. 78° 47′ W.

2. *Distribution of Temperature at the same Depth, on Sections perpendicular to the Axis of the Gulf Stream.*

Diagram No. 4 contains the results of observations on the section perpendicular to the stream, from Sandy Hook, and shows the mode in which the observations were discussed. The positions where the temperatures were observed are marked at the head of the diagram, and above them the distance from Sandy Hook in nautical miles. The temperatures are marked on the side of the diagram. At each position a diagram was drawn similar to those of Nos. 1, 2, and 3, and from the curve traced with a free hand among the points the results at the several depths, which are shown on diagram No. 4, were obtained. The curves traced among these points, so as to preserve as far as possible consistent results for the various depths, are those given in the diagram. In the preliminary discussions of the results I used the observations themselves at the different depths, and am therefore enabled to say, that, while the curves present fewer irregularities by the last mode of discussion, as might be expected, the general results are not in any essential particular changed by its adoption. The diagram shows the curves of distribution of temperature at thirteen different depths, from the surface to 500 fathoms. The depths at which observations were taken were the more numerous nearer the surface, where the changes of temperature were most rapid. These curves were next separated into groups following the arrangement which seemed best to apply to the sections generally. On the Sandy Hook section, for example, as shown in diagram No. 5, the results from the surface to 30 fathoms, from 40 to 100 fathoms, at 200 and 300 fathoms, are grouped respectively in the curves *n*, *o*, and *p*, and that for 400 fathoms is given in curve *r*.

The point where the axis of the Gulf Stream, or line of highest temperature, is cut by the section, is distinctly shown on the diagram, and the minimum of temperature, or "cold wall," within thirty miles of it nearer the shore. These are the prominent features in every case. Farther from shore than the axis of the stream, the Sandy Hook curves show one point of maximum and two points of minimum temperature. In the comparatively cold water of the in-shore counter current, two maximum points, and one minimum, are also distinctly marked. This diagram is, in fact, a general type of the results, according to which the ocean in and near the Gulf Stream is divided into successive warmer and colder bands. The number and the general arrangement of them can, of course, only be made out by a comparison of the several sections. In the discussion such a diagram was drawn for each section.

The curves of diagram No. 5 do not at all indicate at what depth the temperature would approach to equality across the section.

The corresponding results for the Cape Henry section are shown in diagram No. 6 *bis*. The first or "cold wall" minimum, the axis maximum, and two minima and two maxima beyond the axis, are well made out in all the groups from the surface to 400 fathoms. The mean of the results at 0, 5, 10, 20, and 30 fathoms, is shown by the curve *u*, that of 50, 70, 100, and 150 fathoms by *o*, and of 200, 300, and 400 fathoms by *s*.

Not to multiply the diagrams too much, I have omitted those for

Hatteras and Cape Fear, which, besides, have nothing very specially characteristic in them, unless it may be that the Hatteras section is one where the results are most disturbed.

The next diagram, No. 7, shows the results of the Charleston section. The groups *m*, *n*, and *o*, from 5 and 10 fathoms, 20 and 30 fathoms, and 20, 30, and 50 fathoms, resemble each other very much; *p*, the mean of 70, 100, and 150 fathoms, is slightly irregular, and so is *r*, the 400 fathom curve. The curves from 5 to 50 fathoms show extremely well the division of the stream, the first or "cold wall" minimum, the first, second, and third maxima, and the second and third minima. In-shore from the first minimum is a maximum. The irregularities in these observations, though obtained with registering instruments at such considerable depths, are less than those which stationary thermometers sunk in the ground show in the passage of heat to them.

Diagram No. 8 represents the Canaveral section, where the same results which have been stated are shown, but on a very much diminished scale. The observations were not carried far enough from the shore to reach the second maximum. The first and second minima, and the axis maximum, are well marked.

I have omitted, for reasons already stated, the diagrams of the other sections. The same phenomena are in general repeated in all the sections.

The permanency of the division of the stream, and the accuracy and the sufficiency of the observations, may be tested in two ways. The first, by comparing the results of running the same section in different years, by different observers. The second, by the consistency or inconsistency of the results obtained at one depth when compared with those at other depths.

In order to compare the results of different years, some one section was explored in the successive seasons. Thus the Cape Henry section connected the work of Lieutenants G. M. Bache, S. P. Lee, and R. Bache, having been explored by each one; the Hatteras section was common to the work of Lieutenants R. Bache and J. N. Maffitt; and the same Charleston section was intended to be run by Lieutenants Maffitt and Craven.

The Cape Henry section was run over three times, and it appears, by comparing the results of each season with the mean of the whole, that in each group of observations, represented by one of the curves of diagram No. 6, there is an uncertainty of rather less than seven miles in the determination of the maxima and minima generally. The best determined points are the first and second minima and maxima, the "cold wall" minimum and axis maximum having an average probable error of five and a half miles, and the other three points an average probable error of eight miles. This accordance is satisfactory, whether viewed in relation to the probability of recognizing the band in passing through it, or in reference to the determination of the positions of the observations themselves, or to the distances apart of the positions. Diagram No. 6 represents, by different full and dotted lines, the results of the three years' observations on the Cape Henry section, showing, with differences which might be expected, the same general division of the stream. The observations were made during the summer season, partly for nautical reasons, and also as giving a nearer approach to equilibrium than the winter. The differences in the temperature of the whole mass of water during different seasons are often more considerable than the difference in distribution.

The second test of the probable accuracy of determination of the several principal maxima and minima was by a comparison of the independent determination of the maximum and minimum points in the curves of distribution at the same depth, corresponding to various depths from the surface. It was first established by a general induction, that all the points of maximum and minimum, except the "cold wall" minimum and axis maximum, are probably, as a rule, vertically over each other. Next the curve was found by which the recession of the first minimum and maximum from the shore, as the depth increased, could be represented. The difference, then, from the mean curve of recession gave for the first two points, and from the vertical line, or average position, for the other points, the probable discrepancy of determination. It would be out of place here to give all these labored details. This discussion gives, as might be expected, smaller probable errors than the other, for this takes in accidental errors only, and that includes real changes. The mean probable error of determination of the first four points from this investigation was, for the Cape Henry section, for the mean of the three years, one mile, and of the other three points, rather more than two miles. This includes three determinations of which each result is the average.

The corresponding results for all the sections are given in the following Table, and show on the average less than one mile of uncertainty for the mean determination of the first or "cold wall" minimum, two miles and a half for the first or axis maximum and the second minimum, and four miles for the next three points, and about eight and a half for the fourth minimum, which was shown on but three of the sections.

Table showing the Probable Uncertainty in Determination of the Maximum and Minimum Points.

NAME OF SECTIONS.	UNCERTAINTY IN MILES.						
	"Cold Wall," or First Min.	Axis, or First Max.	Second Min.	Second Max.	Third Min.	Third Max.	Fourth Min.
Sandy Hook, .		.75	1.00	3.94	7.99		
Cape May, . .	.82	1.25	2.54	1.57		4.03	4.87
Cape Henry (3 yrs.)	.84	.61	.55	1.70	1.06	.94	3.42
C. Hatteras (2 yrs.)		6.77	6.36	9.31	5.69	6.23	
Cape Fear, . .		1.25			2.98	3.49	13.37
Charleston, . .	1.25	1.57	.72	2.09	2.40	.82	
St. Simon's, . .	.00	.74	1.27	.41			
St. Augustine, .	.52	.51	.44	.44	.55		
Cape Canaveral,	.95	1.69	.39				
Final Value, . .	.83	2.49	2.49	4.00	4.01	3.71	8.45

The Hatteras section presents, as before remarked, the most considerable discrepancies in its results, incident most probably to the nature of the phenomena themselves in that region.

3. *Connection of the Figure of the Bottom of the Sea with the Distribution of Temperature.*

The discovery that soundings could be carried nearly across the Charleston section of the Gulf Stream, and that, after losing them on this section for a short distance, they were reached beyond the axis of the stream, was communicated to the Association at the Cleveland meeting, as resulting from the observations of Lieutenants J. N. Maffitt and T. A. Craven, United States Navy, Assistants in the Coast Survey. The connection between the figure of the bottom and the divisions of the stream which the observations of these officers established as applicable to the section south of Charleston, is illustrated in Diagram No. 9, in which the curves of equal temperature, the depths corresponding to them, and the figure of the bottom, are given. In the diagram the distances from the shore are marked in nautical miles on the top, as also the positions at which the temperatures were explored, and the depths in fathoms are stated on the left-hand side. Each curve is marked with the temperature by Fahrenheit's scale which it indicates, and the curves are drawn for every five degrees for 57° to 77° Fahrenheit.

The bottom of the sea slopes gradually on this section for some fifty miles, reaching a depth of about 20 fathoms, then more rapidly to about sixty-five miles and the depth of 100 fathoms, and suddenly falling off to a depth greater than 600 fathoms. At about one hundred miles from the shore the depth is 300 fathoms, a ridge with a very steep slope on the in-shore side, and a little less so to seaward, occurring 1,500 feet above the hollow to seaward of it, and distant about twelve miles from it. A second rise of 500 feet on a base of twelve miles is followed by a depression of 300 feet on a base of fifteen miles, and then by a gentle slope upwards.

It is altogether probable that all the depths found by observation are greater than the actual ones, but the bottom was brought up in the several cases, showing that the lead had reached it, and it is most probable that the proportions are not far from correct.

The close conformity of the curves of temperature to those of the bottom is obvious from an inspection of the diagram. The descent of the curve of 57° in the deepest part of the section is a remarkable feature, not obliterated in the curves above it, but reaching upward to the surface. In the midst of general coincidences there is one discrepancy which indicates that there may be other causes which produce the distribution of temperature in warm and cold bands beside the figure of the bottom. Further observations will show if this is so, or whether it is an error of observation.

We see in this diagram the cold water passing towards the shore side, following the form of the bottom along which it lies, and forcing the layers above it to take the same general conformation.

On the west of the steep Cape in the St. Simon's section there is a forcing up of the cold water to a considerable height, as is shown less distinctly at position No. 1 of the Charleston diagram. This corresponds to the cold water in these sections.

Whether this remarkable discovery may be the clew to the general distribution of temperature in the Gulf Stream on the deeper sections north of these, is well worth examining, and instructions have been given accordingly.

4. *The Cold Wall.*

It is difficult to fix the depth of the Gulf Stream current, though easy to see from the observations that it is comparatively superficial, extending certainly on the Charleston section, for example, to a less depth than 300 to 500 fathoms, and resting upon cold water belonging to a much higher latitude than that in which it is found. Off Cape Florida, about twelve nautical miles east from the light-house, at the depth of 550 fathoms, the temperature was but 49° Fahrenheit in June, 1853. The mean temperature of the coldest month in the year, on the coast in the same latitude with the point of the axis of the Gulf Stream at the surface on the Charleston section, is about 53½° Fahrenheit. At Key West the mean temperature of the coldest month of the year is, from the Report of the Surgeon-General of the United States Army, 69°.3. Deep-sea temperatures of the ocean generally are required for determining this and other questions of a similar kind.

The lateral limits of the stream are now easily defined, especially in the northern sections, where the change is so sudden from the warm water of the Gulf to the cold stream inside of it towards the shore, that the cold stream was likened, by Lieutenant George M. Bache, to a "cold wall" confining the warm water. The diagrams of the Sandy Hook section, Nos. IV. and V., show this sudden change very strikingly, between positions 13 and 14, the probable minimum being, however, inside of 13. So also the Cape Henry diagrams Nos. VI. and VI. *bis.* The cold wall minimum and axis maximum are shown in Diagram X., on the same scale of miles at the top of the diagram, and temperatures at the sides. That the cold wall exists south of Hatteras is found by the same diagram, when the Cape Fear, Charleston, and St. Simon's sections are compared with those for Cape Henry, Cape May, and Sandy Hook. The difference of temperature is less for the southern sections, but it is still strikingly marked.

In the cold water in-shore from the Gulf Stream, Acting Master Jones, of Lieutenant Maffitt's party, found a current setting southward, as also in the cold band outside of the axis. These results, if shown to be permanent, will be in the highest degree important. As it is, the existence of them at any time shows the cause of many anomalies noticed by navigators in relation to the current of the Gulf Stream.

5. The investigations relating to currents remain to be made in detail, though some results have already been procured. It is important, in work like this, to confine the special attention of observers to a few problems at a time, that they may receive close examination.

As the warm water of the Gulf Stream flows inward and outward from the axis at and near the surface, the stratum, as a general rule, becomes thinner. The current is then outward from the axis, as well as onward.

The changes of the position of the remarkable points in the sections with the season and other circumstances, are undergoing investigation, some results having been already collected.

6. *Chart of the Gulf Stream.*

The alternate bands of warm and cold water into which the ocean in and near the Gulf Stream is divided, are shown in the chart now presented, as deduced from the discussion already referred to. The higher temperatures are represented by the darker shades. The axis of the stream is marked by the darkest full line, and the axis of the colder and warmer bands on each side of it by thinner lines, distinguished as stated on the chart.

The axis marked A, where it crosses the Sandy Hook section, is seen to take the general direction of the trend of the coast, which is even more closely followed by the cold wall axis, which crosses the Sandy Hook section at B. These lines are drawn with a free hand among the points by which they would be rigidly determined in the several sections, so as to give a general consistency to their form. The variations from the points rigidly determined are generally of the same order with the probable errors of these points. On the Sandy Hook section, the first D corresponds to the second minimum of temperature, C to the second maximum, F to the third maximum. The probable limit of warm water is marked on the chart. Within the cold wall minimum is a band of higher temperature crossing the Sandy Hook section at F, and generally well marked, followed by a minimum which appears pretty well determined on the northern sections.

The limits of the chart show the limits of the Gulf Stream explorations up to the summer of 1853, inclusive, the work being still in progress.

5. ON THE PERIODIC AND OCCASIONAL PERTURBATIONS OF THE DECLINATION AND DIRECTIVE FORCE OF THE MAGNETIC NEEDLE.

By PROFESSOR W. A. NORTON, of Yale College.

IN a paper published in the Journal of Science, on November 1st, 1847, I developed a new theory of Terrestrial Magnetism, in which it was taken as a fundamental principle, that the intensity of the magnetic force of a particle of the earth's mass in its action upon the magnetic needle was proportional to the temperature of the particle. This principle was adopted, not from any supposition of *identity* in the nature of the two principles of heat and magnetism, but under the idea that there was probably a *similarity* of nature, and therefore that the laws of their distribution were probably similar, although in all probability having some points of difference. I was not disposed to urge it as a *physical*, but simply as a *mechanical* and *representative* theory. I proceeded to investigate formulæ for the calculation of the declination and the horizontal and vertical directive forces of the needle, and then subjected them to the test of comparison with the results of observation. I found a close agreement to subsist over all parts of the earth with the exception of the torrid zone. To adapt the formulæ to places in the vicinity of the equator, they had to be empirically modified.

In the Journal of Science for November, 1849, I published another paper, in which I undertook to explain upon the same mechanical theory the diurnal variations of the declination and directive force of the magnetic needle, — taking the laws of these variations as they were graphically represented in the Report of the Magnetic and Meteorological Observations made at the Observatory at Philadelphia, during the years 1840 to 1845, under the superintendence of Professor Bache. I soon observed an interesting law of dependence between the diurnal variations of the horizontal directive force of the needle and of its declination, which may be briefly stated as follows. When the curve representing the variations of the horizontal force is convex upwards, the north end of the needle is moving toward the east, and when it is concave, the same end of the needle is moving toward the west; and the epochs of change in the deviation of the movement fall very near the points of inflexion of the curve. It was plain, therefore, that the explanation of the variations of declination was dependent upon that of the variations of horizontal force. It may be well to remark here, that the curve of diurnal variations of the horizontal force has two maxima and two minima. The maxima occur about 4 A. M. and 4 P. M., and the minima about 10 A. M. and 10 P. M. It may be conceived, for the sake of more ready comprehension, as a flood and ebb tide in the ethereal magnetic sea, — the full flood tide lagging behind the sun about four hours, both at noon and midnight, and the full ebb tide following the flood at an interval of about six hours. But it is to be observed, that the fall and rise during the night is generally slight. The rise of the force from 10 A. M. to 4 P. M., and fall from 4 P. M. to 10 P. M., found a plausible explanation in the rise and fall of the temperature; but the slight variations of the force during the night, and especially the rise of it during the latter part of the night when the temperature was falling, and the fall from 4 to 10 A. M. when the temperature was rising, seemed to be at variance with the theory. In attempting to reconcile the two, my attention was called to the fact, that the fall of temperature, in all seasons of the year, slackens very much during the latter part of the night, although theoretically the radiation of the earth was constant at all temperatures, and I thought it probable that the cause of this diminution was also the cause of the rise of the force occurring during the same hours. After a somewhat elaborate discussion of the different supposable causes, I came to the conclusion, that the true cause might perhaps be the deposition of dew, increasing, as it was known to do, during the latter half of the night. It appeared upon inquiry, that the average amount of dew deposited at night was sufficient to have, by the insensible heat given out, a material influence in reducing the fall of temperature; and it appeared also that, upon the theory adopted, the fall of dew might have the effect to augment the horizontal force of the needle; either by simple subsidence, supposing its particles magnetic, or possibly by a disturbance of the electric equilibrium at the place of deposition. This view of the subject led to the idea, that the morning decrease of horizontal force was probably due to the evaporation occurring at the morning hours, and supposed on the average to be the greatest at those hours; and it appeared that there were not wanting coincidences to favor this idea. It derived an apparent support, also, from the fact, that the morning hours of maximum and minimum of the horizontal force were also those of minimum and maximum, both of the electric tension of the atmosphere and of its barometric pressure; since the morning variations of the electric tension and of the barometric pressure are believed to be due to the fall and rise of atmospheric vapor.

Quite recently I have undertaken, by a detailed examination of the Reports of the Magnetic and Meteorological Observations made at Toronto, to satisfy myself whether any direct connection could be traced between the disturbances of the needle and meteorological changes of any kind. The disburbances considered were both the periodic and irregular, so called. The result which I have to announce is, that I have not as yet been able to make out the existence of any material connection, and am persuaded that the needle is but slightly influenced by the ordinary meteorological changes occurring at the station of the needle, whether these be variations of temperature, of humidity, of cloudiness, or of amount of rain, and occurring before or after the hour of magnetic disturbance. It is barely possible that the meteorological phenomena occurring at distant places may have a material influence; but to settle this question would seem to require a more extended system of observations than has yet been made. It is not improbable, too, that a more thorough discussion of the Toronto observations than I have hitherto been able to make might reveal the fact of the existence of a slight influence exerted by atmospheric changes upon the needle. We must, therefore, renounce the attempt to explain the perturbations of the needle by variations of temperature, humidity, &c. If the principle of terrestrial magnetism exhibits grand features of correspondence with that of heat, in its normal aspect, when we come to a nearer point of view dissimilarities stand out in bold relief, and we find we have gone far enough on the road hitherto pursued, and have no alternative but to retrace our steps, and endeavor to reach our goal by some other path.

In this stage of the inquiry comes the announcement, that M. Schwabe, of Dessau, in Prussia, after twenty-five years' close observance of the sun's surface, has discovered a periodicity in the number and magnitude of the sun's spots (the period being about ten or eleven years), — a most significant and interesting fact certainly, in an astronomical point of view, since it places the sun on the list of variable, periodical stars. But we have another announcement from another philosopher, who has been studying the daily movements of the magnetic needle, as they have manifested themselves during a like number of years; that these movements have a period coincident with that of the sun's spots, and that they are the greatest during the year in which the spots are the largest and most numerous, and the least during the year in which the spots are smallest and least numerous. This coincidence points with peculiar significance in the terrestrial, as M. Schwabe's discovery does in the celestial direction. It indicates the path that leads to the unknown region of magnetic disturbance. Let us undertake to follow it; but first let us understand our present stand-point, and take our bearings. The immediate question is, where is the seat, the abiding-place, of the cause of daily magnetic disturbance. We have seen that it cannot be in the region of ordinary meteorological phenomena; it must, therefore, be found either in the crust or general mass of the earth, or in the upper regions of the atmosphere, or external to the atmosphere. Or it may be coextensive with the entire earth, — its solid, liquid, and gaseous matter combined, and the atmospheric stratum of meteorological phenomena may be too insignificant, in comparison with the entire region of disturbance, to have a sensible influence. Still further, it may be supposed to be located in the sun himself. Let us consider the latter supposition for a moment. If the sun acts upon the needle directly, as a great magnet, then its magnetic poles must be nearly coincident with its poles of rotation; since, if they were at any considerable distance from these points, by the rotation of the sun each of the magnetic poles would sometimes be turned toward the earth, and at other times away from it, and hence there should be a very distinct period of disturbance of the needle coincident with the period of the sun's rotation, whereas no such period has been recognized. Again, if the sun's supposed magnetic poles coincide with the poles of rotation, then, when the earth is near the position answering to the vernal equinox, its northern hemisphere will be most affected by the south pole of the sun; and when it is near the other equinox, it will be most affected by the north pole. Toward noon there ought, therefore, to be an increase of horizontal force at one of these epochs, and a decrease at the other. If we suppose, for a moment, that one of the poles of the sun is more efficient than the other throughout the year, then the effect at sunrise and sunset upon the horizontal force will be opposite in summer and winter, since at these seasons the sun rises and sets on opposite sides of the east and west points of the horizon. If any one should imagine that the sun might by direct action alter the magnetic condition of the needle, I answer, that experiment has failed to establish the existence of any such effect; and besides, from such action variations of horizontal force alone could result; it would still be necessary to provide some other cause for the daily movements of the needle.

I conclude, therefore, that the periodic disturbances of the magnetic needle cannot be attributed to any direct action of the sun upon the needle. Before considering the question, whether the seat of daily magnetic disturbance may not lie below the earth's surface, let us inquire into the probable nature of the sun's action upon the magnetism of the earth. This may either be a *direct polar action*, or by the intervention of some *emanation*. Now, if we suppose a direct polar action, we fall upon this difficulty, — the tendency of this action would be to make the maximum (or minimum) intensity of the earth's magnetism, and of the horizontal force of the needle during the day, fall at sunrise, noon, sunset, or midnight, according to the season of the year; since the continued tendency would be to develop a pole at that point of the earth's surface which is 90° distant from the ecliptic, and this point would move entirely around the polar circle in a year.

It would seem, therefore, that we are driven to the inevitable conclusion, that it is some emanation from the sun which produces the effects under consideration. Now, whether this emanation be some species of ray, or some form of magnetic matter emitted from the sun, it is highly improbable — being in opposition to all analogies — that it can penetrate the earth's atmosphere, and the mass of the earth to a depth below the stratum of variable temperature, and modify the magnetic forces whose centres may be located below that depth. What, then, is the probable conclusion that remains? It is this: that the seat of the principal disturbing forces is in the upper regions of the atmosphere, or the space immediately exterior to the atmosphere, and that these are excited by, or consist in, some emanation received from the sun. But the question still remains, Is this emanation probably some species of ray, — calorific, colorific, tithonic, — or some species of magnetic matter? There is an obvious objection to the supposition that it is a ray. From all that we know of the absorptive power of the atmosphere for the sun's rays, the amount of any species of ray absorbed in the upper regions of the atmosphere must be very small, and therefore its power of disturbing the magnetic forces of the highly rarefied air of those regions, or of any subtile magnetic matter by which they may be pervaded, should be very slight. We therefore infer it to be highly probable, that the *emanation from the sun consists in some form of magnetic matter*, and the final conclusion to which we arrive is: —

That the explanation of the daily periodic perturbations of the magnetic needle is to be sought for in the highly probable fact of the daily arrival of magnetic matter emitted from the sun, at the surface of the atmosphere. That some subtile form of magnetic matter is occasionally present in those elevated regions has long been admitted to be a fact, — ever since the discovery was made that the Aurora Borealis disturbed the magnetic needle, and that its beautiful streamers were parallel to the dipping needle, and its arches of luminous matter perpendicular to the magnetic meridian. The great perplexity has been to account for its existence. We now see the source of this mysterious substance. It comes to us from the sun, from whose fiery surface it is perpetually streaming toward the earth. I make this statement without hesitation, — in no spirit of daring speculation, but as a legitimate conclusion arrived at by a careful process of induction. In the generalization thus arrived at, we recognize another link between the earth and the sun, and we shall see, as we proceed, that it is radiant with a golden light that beams forth in all directions. While it appears to me to wear the aspect of a physical truth, my present intention is simply to advocate it as a representative theory, embracing, within its ample scope, a wide range of diverse phenomena.

Let us first endeavor to form some idea of the probable nature of this magnetic substance. Shall we suppose, in accordance with the ideas of some philosophers, in regard to the nature of the matter of auroras, that it is a highly subtilized vapor of iron? The fact that meteoric stones contain so large a share of iron, and (in view of the truth just stated, that the substance is continually arriving from the sun) the fact of the general presence of iron in small quantities as a constituent of the materials of the earth's crust, may seem to some persons to favor this hypothesis; but the supposition seems to require the existence of a temperature so exceedingly high, that it has the air of great improbability. It is true that it issues from a fiery furnace, but we should expect that, after remaining, as it must often do, for several hours at least, and probably for days, in the super-Siberian region in which its fires are kindled, its temperature would fall far below furnace heat. Besides, if condensed vapor of iron is constantly subsiding into our atmosphere, ought not the delicate chemical tests we possess to reveal the constant presence of iron in rain-water? On the other hand, if we admit the existence of permanent magnetic particles of iron in the supernal regions of the atmosphere, and that the air is positively electrified *from the earth*, and that the intensity of its electrical charge increases upward, we apparently have a sufficient basis for a satisfactory theory of the Aurora Borealis. But it is to be observed, on the other hand, that it is by no means established that any or all of the processes going on at the earth's surface are adequate to the supplying of the atmosphere with its charge of positive electricity. Becquerel and other experimenters deny that electricity is evolved in the act of evaporation. But if the atmosphere is not electrified by terrestrial processes, then we must look to the sun as the source of its electricity. Now, as the principle of magnetism is but one form of that of electricity, the idea suggests itself that the magnetic matter received from the sun, which is the probable source of the disturbances of the magnetic needle, *may also be the source of atmospheric electricity.* On the whole, I incline to the opinion that the solar magnetic matter is not the subtilized vapor of iron, or of any other magnetic substance, but *pure electric ether*, the same which, accumulated around the particles of bodies, is the common source of all electrical and magnetic phenomena, — a form of matter, as I conceive, much more dense than the ether which pervades space, and which is subject to the polar forces of ponderable matter. To the region above the earth in which this magnetic substance derived from the sun is found, whether it be wholly exterior to the atmosphere, or partially or entirely within its surface, I shall give the name of the Photosphere of the Earth. The propriety of this designation will be apparent, when we come to turn our attention, as I propose to do, in another paper, to the subject of the Physical Constitution of the Sun. I would not imply by this term, that the magnetic matter referred to is necessarily everywhere in a statical condition; that is, that it forms a vast ethereal stratum in a state of rest and permanency, except as its normal state is altered by the arrival of new matter from the sun when he is above the horizon, and by the incursion of auroras descending from the polar regions. This I conceive to be the most probable supposition; still, except in the vicinity of the magnetic poles (the abiding-place of the auroral matter), the matter of the photosphere may all be in the act of flowing away from the point immediately under the sun, during the day, and during the greater part of the night may be wanting altogether. It will soon be seen that there are apparently good reasons for dividing the photosphere into two strata, like the two strata, self-luminous and non-luminous, which encircle the sun, — an upper one in a frequent, perhaps constant, dynamical condition, and a lower one habitually in a comparatively statical condition.

Before attempting to make out the explanation of magnetic disturbances, it is necessary to frame a consistent theory of the Aurora Borealis, which is one recognized cause of disturbance. In the first place, then, I conceive that the electric or magnetic ether received from the sun, on its arrival at the photosphere, consists probably of a bundle of particles, each of which is a little magnet, or has a north and a south pole. These particles are arranged in a continuous line, being held together, though not in contact, by the attraction of dissimilar poles. The probability of this supposition will appear in another connection. It will answer equally well, however, from most points of view, to suppose that they arrive in detached particles. So soon as they have become a part of the photosphere, the line of their poles will take a position parallel to the dipping needle, under the action of the earth, and they will be arranged in lines parallel to this direction. The north poles (similar to the north pole of the needle) will be downward. This arrangement will extend throughout the entire depth of the photosphere, and be prolonged through the atmosphere by a chain of polarized particles of oxygen, which Faraday has shown to be susceptible of ordinary magnetic polarization. It will also extend to an unknown depth into the mass of the earth. These lines are equivalent to what Faraday calls the lines of magnetic force. It is, as I conceive, *upon a statical view of the subject*, the action of the entire mass of these polarized lines, within a certain unknown distance of the needle, — extending, be it observed, from the top of the photosphere through its depth and that of the atmosphere, and probably through the crust of the earth, — that constitutes the normal magnetic action of the earth. The lines of particles which have just arrived from the sun will flow off in a direction from the sun, and separate by the mutual repulsion of similar poles. They will, therefore, flow towards the north and west during the forenoon, and north and east during the afternoon; except early in the forenoon and late in the afternoon during those months in which the sun is north of the equator, when they will tend southward. The continual flow of such particles toward the north while the sun is north of the horizon will cause an accumulation there, and when lines of considerable length come to be formed they will be sensibly attracted by the north magnetic pole of the earth, and accumulate over and circularly around this point. The lines of polarization, extending down to the earth's surface, will therefore be longer and more intense in this region than elsewhere. The polarization at the earth's surface, induced by this polarization, propagated downward in vertical lines through the atmosphere, will therefore also be intensified. As the night wears away, the increase of humidity that occurs in the lower regions of the atmosphere tends to bring about a discharge, if it does not occur before. At a certain point of time such a discharge takes place; that is, at the earth's surface, or at some higher point in the line of polarization, two contiguous poles rush toward each other. This highly augments the polarization throughout the whole line, and is attended with the evolution of light, and probably heat, throughout its length. I *explain the light in this manner*. This electric ether is the same that encircles the molecules of bodies, and which, when set in vibration, gives rise to waves of light in the great ethereal sea that pervades all space and all bodies. Now, the vibration consequent upon the electric discharge between any two contiguous poles will be propagated successively to the other particles in the line, and light is evolved from every point. This is commonly expressed by saying that an electric spark passes along the chain of particles, and the supposed phenomenon which I have just described is what would, in ordinary phraseology, be called an electric discharge between a positive and negative surface. The discharge spoken of would be likely to occur at the same time over a considerable extent of surface, and thus we should have an auroral column,—an auroral streamer; and if a simultaneous discharge were to take place over several isolated districts, by reason of local peculiarities or from any fortuitous cause, several distinct columns would be formed. These columns being electrically excited, and having their polar forces intensified, would have a violent repulsion for each other; and the more southerly ones would, in consequence, move off rapidly toward the south. The tops of the columns, extending doubtless above the surface of the atmosphere, may become detached, and move off more rapidly southward. Being perhaps many miles in length, they would be drawn backward and downward by the magnetic force of the earth. The initial direction of motion, being parallel to the earth's surface in the polar regions, would tend to carry them away from the earth as they moved southward. How far they will retreat toward the equator must depend upon the intensity of the original impelling force, that of the retarding force of the earth, and any resistance they may encounter from the atmosphere, or the photospheric matter exterior to the atmosphere. Short columns emerging from the surface of the photosphere may pass off into space, and leave the earth entirely. When the motion of the retreating columns ceases, they will tend to fall into the atmosphere, and relapse back toward the pole. If already within the atmosphere, the descent and retreat will be slower, and they may very well be broken up into smaller columns, forming stationary clouds near the surface of the atmosphere, as we not unfrequently see in the breaking up of an aurora. This phenomenon may also occur in the lower columns that are subject to most resistance, before the relapse and subsidence of the higher columns begin. The continued phosphoric luminosity of such clouds, and sometimes of moving columns, may either be the gradual subsiding of the vibratory motion that has been excited, or the incessant play of electrical discharges of feeble tension. It is the accumulation of this matter gradually subsiding into lower but still highly elevated regions, until it finds its equilibrium, that forms the lower stratum of the photosphere. As we shall see on another occasion, this in all probability has its counterpart, on an immensely larger scale, in the cloudy stratum posited below the sun's proper photosphere. It is this lower and comparatively quiet ethereal sea that is the probable normal source of the positive electricity in the lower portions of the atmosphere, and of the negative electricity induced at the earth's surface.

It is to be observed, that, as the auroral columns descend from the polar regions, electric discharges may still continue to occur along them by reason of their inductive action upon the regions below, and that the liability to their occurrence may very well vary from one period to another, and from one locality to another, with the humidity of the upper regions of the atmosphere.

When a column is immersed in the sea of photospheric matter, the intensification of the polarization of its particles produced by an electric discharge along its length will be attended with a sudden expansion, which will be succeeded by a partial contraction. This conception furnishes a plausible explanation of certain optical phenomena of the aurora. If a column should pass entirely out of the photosphere, it would expand, by reason of the mutual repulsion of its particles, until this repulsion from the action of similar poles is balanced by the attraction subsisting between the dissimilar poles; and, accordingly, would not cease to have an organic existence.

It is implied in the theory that I have now advanced, that the parallel lines and columns of the sea of electric ether which surrounds the earth have a magnetic power. In fact, I conceive that they exert a magnetic action, after the same manner, essentially, as an ordinary magnet (which is a bundle of lines of polarized particles of iron); since I attribute the polarization of these particles to a polar arrangement of the atmospheres of electric ether by which they are surrounded. We here touch upon another field of inquiry, the detailed examination of which must be reserved for another occasion. Let it be understood, however, that the idea of an electric ether is not essential to the explanation of the principal phenomena considered in this and the following paper; as the basis of a representative theory, the only necessary conception is that of some form of subtile magnetic matter.

Let us now see what explanation the present theory furnishes of *Magnetic Disturbances*, both periodic and irregular, so called. The fundamental idea at first to be borne in mind is, that the magnetic particles, or chains of particles, received from the sun, arrange themselves, by the action of the earth, in lines parallel to the dipping needle, with the north poles downward. Now, for greater simplicity, let us conceive that the plane of the magnetic meridian coincides with that of the true meridian. It will be seen, on a little reflection, that from the zenith to the south point of the horizon, and below the horizon to a distance equal to the complement of the dip, say 20°, the north poles of the photospheric particles will be turned toward the needle;

that the same will be true, from the zenith 70° toward the north; but that thence down to the north point of the horizon, and far below the horizon, the south poles will have this position. At the point 70° north of the zenith, the two poles of a particle will be at the same distance from the needle, and it will exert no magnetic action on the needle. Between this point and the zenith, the difference of distance of the poles is less than for particles correspondingly situated south of the zenith. Accordingly, the principal effect will be produced by the particles that lie to the south of the zenith. This is more especially true in these latitudes, as the sun passes the meridian to the south of the zenith, and as most matter must be received, over a given space, directly under the sun. Now, the action exerted by the particles that lie south of the zenith is repulsive upon the north end of the needle, and hence its tendency is to augment the horizontal directive force. It is plain, therefore, that this force ought to increase toward noon. It will soon be seen that there is reason to believe that this solar emanation meets with considerable resistance from the photosphere, or atmosphere, on its arrival. If this be true, then it must continue to accumulate on the meridian for a certain interval of time after the sun's meridian passage, and accordingly the horizontal force to increase.

This would be true if this accumulation did not take place, since the intensity of the force depends upon the entire action of all the magnetic matter above the horizon, and doubtless for a considerable distance below it. But how are we to explain the decrease of this force from 4 to 10 A. M.? I conceive that this effect may arise from the great impulsive action of the solar emanation, at sunrise, and for a certain time afterward, upon the matter of the photosphere overhead, — driving a portion of it farther to the west, diminishing thereby the horizontal intensity, and also occasioning at the same time a rise of the same over more westerly places where the sun has not yet risen. The absence of a rise of force some two hours after sunset may be owing to the fact that the surface of the earth is now moving away from the sun, by reason of which the impulsive action may be very much weakened. The laws which have been made out with regard to the occasional perturbations of the needle seem to indicate that the impulsive action of the solar emanation upon this more intense magnetic matter toward the north (the auroral columns) may have something to do with the regular diurnal perturbations. The effect of any such action ought to be slight at sunset and early in the evening.

It is plain that, agreeably to the theory thus briefly developed, the diurnal variations of the horizontal intensity ought to be greatest at the epoch of the summer solstice, and least at that of the winter solstice; also, that the intensity of the horizontal force ought to be greater in summer than in winter. Now it has recently been established by Colonel Sabine, by a discussion of the Toronto Observations, that these two laws of variation in fact obtain. As to the diurnal variations of declination, I have already, in a previous paper, given a satisfactory explanation of them, entirely independent of all theory respecting the nature of the principle of magnetism, — involving no supposition, in fact, but that the effect depends mainly upon the variation, produced by the sun, of the magnetic forces of the particles lying within a certain region around the station of the needle. Upon any *statical* theory of terrestrial magnetism, the needle must always tend to place itself in a direction perpendicular to the mean isomagnetic line passing through the station of the needle. I mean, by the mean isomagnetic line, an ideal line traced through the station of the needle and a series of points, where the mean intensity, in its relations to a horizontal needle, of all the particles throughout the entire height and depth of the ethereal sea of magnetism, is the same. To indicate the theory briefly, suppose we take the hour of 11 A. M.; at this hour, at the station of the needle, the magnetic intensity of the particles in the vicinity of the needle is on the increase, but to the east of this station the increase is more rapid; hence, as the normal horizontal action of the magnetic particles decreases toward the north, it is plain that to the east of the station of the needle the isomagnetic line must shift its position toward the north, and the needle move toward the west. Thus the relation between the rate of variation of horizontal magnetic intensity at the station of the needle and at a place not far to the east or west of it, must determine whether the needle will move in one direction or the other; and therefore the epochs of change of direction should be coincident, or nearly so, with the points of inflexion of the curve showing the daily variations of the horizontal force.

The discovery recently made by Sabine, that the north end of the needle at Toronto is farthest to the west at 1 to 2 P. M. at the northern solstice, and declines least in that direction at the southern solstice, is apparently a consequence of the fact that the hourly increase of the horizontal force from previous to 1 P. M. is the most rapid at the one epoch, and the least so at the other. The reverse annual movement of the needle, at the hour of 8 A. M., I ascribe to the annual variation in the morning hourly decrease of the horizontal force.

At Hobarton, in the south temperate zone, the south end of the needle moves according to the same laws as the north end in the north temperate zone, and the explanation for the one holds good for the other.

The observations at St. Helena have revealed the existence of a law at that station which does not obtain either at Toronto or Hobarton. It is this. When the sun is north of the equator, the daily movement of the north end of the needle is the same that it is at Toronto throughout the year, and when the sun is south of the equator, the movement of the south end is the same that it is at Hobarton. To explain this anomalous fact, suppose for the moment that St. Helena is on the equator, and that the magnetic and geographical equators are coincident. Now, when the sun is over the equator, the daily variations of horizontal magnetic intensity will be the same both to the north and south of it. There will, hence, be two isomagnetic lines passing through the station of the needle, inclined under the same angle to the equator, and during the day these lines will shift their position to the same amount and in the same direction, — that is, both move toward or from the equator at the same time. Hence at the equinox there can be no daily movement of the needle. But if we suppose the sun to get to the north of the equator, then the daily variations of horizontal intensity become greater at places to the north than at places to the south of the equator, and hence the north isomagnetic line shifts its position more than the other, and accordingly controls the movements of the needle. When the sun is south of the equator, the same will be true of the south iso-magnetic line. Now, as a matter of fact, St. Helena is south of the geographical equator, and also somewhat to the south of the magnetic equator; but from its proximity to these lines we might expect that the two isomagnetic lines would exist, and that one or the other of them would prevail according to the situation of the sun with respect to the equator. It would naturally be supposed, however, that the transition from the one to the other would occur when the sun had a considerable southern declination, instead of about the time of the equinox, when the change in the direction of the diurnal movement really occurs. This seeming discrepancy, we think, may be removed by attending to the peculiar circumstances of the case; especially to the fact that at St. Helena the normal isomagnetic line is inclined under a large angle (about 20°) to the east and west line, running from north of east to south of west. On this account, during the forenoon, when the sun is south of the equator, the increase of horizontal magnetic intensity at places considerably to the east of St. Helena will be greatest on the south of the isomagnetic line, and so, of the two daily isomagnetic lines, the southern one may shift its position the most.

Similar phenomena obtain at the Cape of Good Hope, of which a similar explanation may be given.

Let us now briefly consider the so-called *irregular* disturbances of the magnetic needle. These have generally been ascribed to the magnetic action of auroras. Sabine's discussion of the Toronto Observations has brought out the following prominent laws with respect to the occasional disturbances of declination.

"The *easterly* disturbances are *below* the average both in number and value during the hours of the *day*, or from 6 A. M. to 6 P. M., and *above* the average during the hours of the *night*, or from 6 P. M. to 6 A. M.; whilst the westerly are below the average both in number and value from about noon to midnight, and above the average from midnight to noon. The easterly have a minimum both in number and value about 3 P. M., and a maximum about 9 P. M.; the westerly, a minimum about 9 P. M. (at which hour the easterly have their maximum), and a maximum about 7 or 8 A. M. From noon to 6 P. M. are the hours of least disturbance."

It appears to me that a sufficient explanation of these laws is found in the advance and recess of the auroral columns, on different meridians in succession, from east to west; the hour of greatest average advance being toward midnight. It will be recollected that these columns have their elevated or south poles turned toward the needle, until they attain an altitude above the horizon equal to the complement of the dip. At a great distance to the north, however, the increase of dip will tend to bring the two poles of the columns to an equality of distance from the needle; for this reason, besides the diminution of force from greater distance, it would seem that, in general, auroras ought not to influence a needle materially until they have descended from the pole to within a certain angular distance from the north point of the horizon. Now, during the evening, until 9 or 10 o'clock, the greatest advance generally takes place on meridians to the east of the place, and hence, in general, the attractions of the advancing columns ought to draw the needle eastward, and the maximum effect and frequency should be a short time previous to the hour of maximum energy and frequency of auroras at the station. After this hour, or during the latter half of the night, the meridians of greatest advance lie to the west of the station, and the liability to a westerly deviation of the needle increases, and to an easterly declines. The maximum of westerly disturbance occurs at 7 or 8 A. M., probably because the difference of attractive action of auroral columns situated on the east and west side of the station is then the greatest, — this difference having been enhanced toward morning by the fact that the sun tends to drive the columns situated on the meridians to the east of the station toward the west. The afternoon is the hour of least disturbance, because there is then less liability to the advance or recess of auroras than at other hours. In support of these views with regard to the action of auroras, we have the following law of auroral action, deduced by M. Bravais, from observations at Bosekop: "The horizontal intensity increases when the Aurora Borealis appears, or is going to appear; then it diminishes during the most active period of the phenomenon." During the first period the forces should be attractive for the north end of the needle, and during the second, repulsive, and diminishing to horizontal intensity until the lower ends of the columns have passed the prime vertical.

The occasional disturbances attain to a maximum about the equinoxes, the two periods in the year when auroras are the most frequent. The same class of disturbances also, like the daily variations, follow sympathetically the variations in the number and magnitude of the sun's spots, being the most frequent and greatest in amount when the spots are largest and most numerous. Since a connection between these disturbances and auroras is highly probable, we have here another indication that the auroral matter of the earth's atmosphere, or rather photosphere, is probably derived from the sun. But if magnetic matter is emitted from the sun, from what parts of its surface does it probably flow? If the sun is a magnet, this matter must have magnetic relations to the poles of the sun, and be accumulated at the polar regions in greater quantities than over other parts of the surface. At this stage of our inquiry the idea naturally suggests itself, that it may be *auroral matter in the photosphere of the sun, and that the light and heat of the sun are auroral emanations;* that is, that the ethereal waves of light and heat which proceed from the sun are incited by the vibrations of the auroral matter. But the auroral columns within the earth's photosphere proceed from the polar regions of the earth, and flow toward the equator. They are suddenly impelled in a direction parallel to the earth's surface, and the more elevated columns or fragments of columns which pass out of the photosphere recede from the earth, moving doubtless very nearly in straight lines, but subject to a diminution of velocity from the resistance of the ether of space. After the same manner we may suppose that auroral matter streams off incessantly from the polar regions of the sun. If such matter is perpetually in the act of flowing away from the polar regions of the sun into space, it will naturally be asked, Should it not be visible *in transitu?* We reply, that it undoubtedly is; that it is the substance of the *Zodiacal Light.*

To make this clear, we have but to conceive that the luminous matter in question is flowing in the manner just stated from various points of the sun's surface, from the poles down toward the equator. The rectilinear paths of these emanations will cross the plane of the equator at various points, from near the sun's surface to a point probably beyond the earth's orbit. The orbits traced by matter flowing from corresponding points of the surface of the sun in the two hemispheres should intersect the plane of the equator at about the same point, and meet under a certain angle. Again, each orbit traced from any one point will be intersected along its different points by all the orbits proceeding from the various parts of the other hemisphere; hence the light will be more intense along these lines. If we leave out of view, for the moment, the continuation of the luminous lines beyond these points of intersection outward, it is plain that we shall have the sun perpetually encircled by a nebulous mass of a lenticular shape symmetrical with respect to the plane of his equator; that is, if we regard the evolutions from the two polar regions as equal, at corresponding points, in amount and velocity. The section of this lenticular-shaped mass, as seen from the earth after sunset, will have nearly the form of a triangle, brightest toward the middle, with its base at the horizon nearly equal to the diameter of the sun's disc. But the observer does not see a mere section perpendicular to the line of sight, but the half of the whole mass, with the exception of a certain portion near the sun. The part of this mass beyond the section will have the effect to make the section appear brighter than it otherwise would, and more luminous towards the middle line, lying in the plane of the equator. The part in front of the section will have a corresponding effect, but will give to the luminous appearance greater breadth; in fact, if the earth is immersed in the mass, a breadth limited only by our powers of vision through the atmosphere. The apparent breadth of distinct luminosity will be, in fact, much greater than it would be on the supposition we have made, for the rays of luminous substance which stream from either side of the equator, after crossing all those from the other side, proceed indefinitely into space, and thus give greater actual thickness to the luminous mass, and greater apparent breadth to the luminous appearance.*

6. ON THE PHYSICAL CONSTITUTION OF THE SUN AND THE COMETARY BODIES. By PROFESSOR W. A. NORTON, of Yale College.

THE idea of a photosphere exterior to the earth's atmosphere, consisting of a species of subtile magnetic matter, which has afforded us an adequate explanation of the Aurora Borealis, when transferred to the sun and the comets sheds a new light upon their physical constitution. Recalling to mind the ideas we have formed of the photosphere of the earth, and of the real nature of auroral phenomena, we are led to suppose that the luminous matter at the surface of the sun is arranged in luminous columns, or rather irregular masses more or less distinct, and that these masses consist of lines of bipolar particles, parallel to the line of direction of the directive force of the sun at the locality where they exist. These columns, as we shall call them, — although it is important to bear in mind that they are really masses of almost any shape, — will therefore be vertical at the sun's magnetic poles, which we shall take to coincide with the poles of rotation; and gradually deviate from this position until at the equator they become parallel to the surface. When new columns come down from the poles toward the equator, they occupy a region of the photosphere more elevated than the general luminous surface of the sun. The reason of this is, that they have been ejected from the sun's polar regions by a certain projectile force, in a direction parallel to the surface. Issuing from the elevated regions of the photosphere in such directions, they must move off in orbits lying entirely above an imaginary spherical surface concentric with the sun and traced through the starting-point. These columns, moving down from the polar regions toward the equator, are resisted by the surrounding matter of the photosphere, and possibly also by the ether of space condensed around the sun. Columns of considerable length will also be retarded by the polar attraction of the sun. Another cause of retardation may exist when the columns have receded to a considerable distance from each other; for since the attraction between dissimilar poles will be stronger than the repulsion between similar poles, the mutual repulsion may, at a certain distance, be converted into an attraction. Now, the auroral columns which have retreated from the sun's poles thus become disseminated over a certain portion of the surface extending from the poles possibly to the equator, but probably not so far. They will subsequently subside into the solar atmosphere, and attain to a position of equilibrium, in which they will be arranged nearly concentrically with the sun. Between these columns and the electrified atmosphere below them will subsist the relation of bodies in opposite electrical states, which will tend to cause a still greater descent, and more of some columns than of others. Between the two, electric discharges may perhaps take place at any part of the sun's surface, either paroxysmally or incessantly, for a limited time. The pulsations of the particles from this disturbance of electric equilibrium may be one cause of the sun's light, but I conceive the chief cause to have its seat at the polar regions. The electrical disturbance just supposed is at least more energetic there, after the same manner as upon the earth, and it is there that the columns acquire their luminosity. Of this luminosity, that is, vibratory motion of particles, a certain portion remains, and there may be a frequent, as on the earth, or perhaps a perpetual, transmission of vibration, or luminous excitement, from the poles to the equator. The idea that the light of the sun is of electric origin is by no means a new one. It was first suggested, I believe, by Sir John Herschel, who conjectured that this light might be of auroral origin. But no systematic theory on the subject has hitherto been developed, and, so far as I know, no attempt made to explain the phenomena of the sun's spots upon such a theory.

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* In the development of the theories of the Aurora Borealis and Zodiacal Light, it has been assumed that the magnetic ether is not subject to the attraction of gravitation. If we suppose it to gravitate toward ponderable matter, the conception of the details of its deportment in certain cases would be somewhat modified.

A new and more detailed discussion of the magnetic perturbations, based on the *dynamical* theory of terrestrial magnetism, may be found in the March and May numbers (1855) of the Journal of Science.

Sir William Herschel has shown that the dark body of the sun is surrounded by a stratum of non-luminous matter, floating at a great elevation above the surface, but lying considerably below the exterior luminous surface. The existence of such a non-luminous mass in this lower situation is but a simple consequence of our theory. We have seen that the columns which have descended from the polar regions gradually subside into the atmosphere. The final result should be, an accumulation of non-luminous matter at a lower level. If there should be any inferior cause of electric excitement continually existing, this matter would still give some light, and might have something of a columnar arrangement; otherwise it would become entirely non-luminous, and its density would be about the same over a wide district. The presence and varying proximity of the excited columns above it would be the source of inequality of excitement in the lower stratum. It is most probable, therefore, that the columnar arrangement would exist throughout this stratum.

We have now, as it appears to me, the basis of a satisfactory *theory of the sun's spots.* Whenever from any cause certain contiguous columns have their polar forces intensified, they must repel each other, in one or several directions from a central point, as the case may be, and reveal through the opening thus made the non-luminous stratum below. This separation must continue until the point of equality between the repulsion and attraction is reached, or until an equilibrum is obtained between this repulsion and the inward pressure from the surrounding columns. When the excitation declines, or the inward pressure of the marginal columns increases, a collapse will occur. The origin of this excitation I attribute to the arrival of new auroral columns in the upper photosphere. Such columns must excite, by induction, the columns below them, and thus produce a separation. Such new more elevated auroral columns, if at a distance from each other, may also effect a separation of those below, in the space between them, by their unequal oblique attractive action. Repelled from each other, the same columns may increase the opening once made. It is in this manner that I conceive it most probable the opening is generally made in the lower non-luminous stratum; namely, by an unequal oblique attraction of the marginal columns in the opening above, for the upper ends of the columns below, underneath the opening. It is easy to see that when the upper columns, receding outward from a centre, arrive at a certain distance, a separation will begin, in this way, under the centre of the opening, which will widen as the upper opening enlarges, but always appear smaller than this; and thus we have the black central spot (the dark body of the sun seen through the central opening), surrounded by a fringe of uniform shade, — the penumbra, as it is called.

I cannot now enter into a theoretical discussion of the different varieties of appearance. This must be reserved for another occasion. I will simply remark, that the present theory is in beautiful accordance with the later delineations of the solar spots by Capocci of Naples, and Pastorff of Frankfort (on the Oder), in which their serrated edges and minor details are exhibited. It accords no less satisfactorily with the descriptions we have of the general telescopic appearance of the sun's surface, than with the aspects of the spots that here and there, and from time to time, show themselves upon his luminous face. The Herschels describe the sun as having a mottled appearance, which may be compared to a stratum of luminous clouds of varying depth, and having an unequal surface, or to the appearance produced by the slow subsidence of some flocculent chemical precipitate in a transparent fluid, when looked at perpendicularly from above. We have already seen that the auroral masses ought naturally to be at various elevations in the photosphere at different localities, and that the subsidence of a chemical precipitate here figuratively mentioned actually exists, the precipitate being the auroral clouds. Sir William Herschel states, that the sun's surface is broken up into mountainous masses of light, of great height. Sir John Herschel says, that its ground is finely mottled with an appearance of minute dark dots, or pores, which, when attentively watched, are found to be in a constant state of change. What words could be better descriptive of the appearance the dark body of the sun would probably present seen through the interstices of the auroral columns, ever shifting their positions, by reason of their varying reciprocal action?

The *Faculæ*, the brighter streaks upon the sun's surface, are, as I conceive, the auroral columns newly arrived from the polar regions, which should be brighter than the others, and, if not, would enhance the brightness of the region, by the addition of more luminous matter. The greater condensation of columns at some places than at others might occasion a similar appearance. Upon the first view we see why the faculæ should generally precede the breaking out of dark spots, since our theory is that the new auroral columns originate these spots.

The sun's surface presents varieties in its different parts. At the polar regions no spots are ever seen, and the same is true, we are told by recent observers, at the equator, and for a few degrees on either side of it. The spots, it is manifest, could hardly make their appearance at the polar regions, from the want of sufficient separation of the auroral columns. These masses, as they move away from the poles along the meridians, separate from each other. I suppose the cause of their non-appearance at the equator to be, that the descending columns, as on the earth, do not reach the equator.

Some of the curious luminous phenomena exhibited to telescopic observation in solar eclipses find, I think, a plausible explanation in the views now presented. I allude to the *rose-colored flames, or prominences;* the bright *corona* encircling the sun with its beamy and radiated structure, as described by some observers, or its vivid and flickering rays, as we are told by others; and the long jets of light, called *aigrettes* by Arago. I conjecture the rose-colored prominences to be new auroral clouds in the upper photosphere of the sun. It is well known that the auroral streamers which make their appearance in our sky sometimes have the same roseate tint. The ground of the corona may be the exterior photosphere of the sun, or, less probably, its highly attenuated gaseous atmosphere. The rays by which it is streaked may perhaps be the propagation of the auroral action into the upper regions of the photosphere; and the aigrettes, the auroral matter flowing off chiefly from the polar regions into space, and visible at a greater distance from the sun as the substance of the zodiacal light. It is possible, however, that some of these phenomena may have their origin in the earth's atmosphere, as some astronomers have conjectured. I merely throw out the above suggestions that they may receive such attention as they deserve from the astronomers who are conversant with the facts from actual observation.

The physical connection between the sun's spots and the disturbances of the magnetic needle upon the distant earth, through the solar auroras, the zodiacal light, and terrestrial auroras, is fully made out by the theory now presented. If we ask for confirmation from historical facts, neither is this wanting. The elaborate inquiries of Cassini and Mairan have long since rendered highly probable, upon historical grounds merely, the existence of some physical connection between the sun's spots, the zodiacal light, and terrestrial auroras.

I would remark, in passing, that the theoretical views now set forth seem also, as might be expected, to shed a new light on the diversified phenomena of variable stars, which have hitherto proved so perplexing.

Let us now advert very briefly to the subject of the *physical constitution of comets.* In a paper read before the American Philosophical Society at their Centennial Celebration (1844), I advocated the theory, that the matter of the tails of comets was continually in the act of flowing away from the head, and showed that this theory was perfectly consistent with all the established facts with regard to the position and form of the tail of a comet. The force of expulsion I attributed to some repulsive or impulsive action of the sun. In the light of the present theory we have, as I conceive, a revelation of the nature of this action. We see that it is the impulsive action of the auroral matter streaming from the sun. But we are provided with another cause of expulsion of cometary matter in the same impulsive force in operation in the photosphere of comets that we have seen to be in operation at the polar regions of the earth and sun.

I conceive that a comet consists of a mass of ordinary ponderable matter surrounded by an atmosphere, and also by a photosphere of magnetic ether of great extent. This photosphere I suppose to be the nebulous envelope, — the coma. The nucleus, whether composed of solid, liquid, or gaseous matter, may be supposed to have two magnetic poles, like the earth. Under the influence of these poles the bipolar particles of the coma will tend to arrange themselves everywhere in lines parallel to the directive force of the nucleus, and to accumulate about the poles. The nucleus and coma will also have magnetic relations to the sun. When a comet, descending toward the sun, comes within a certain distance, it will turn one of its poles toward this luminary; the line of its poles tending to take a position parallel to the sun's directive force. Now, the line of the poles being supposed to have this position, conceive the same auroral phenomena to occur over the pole nearest the sun as happen in the photosphere of the earth, under a similar constitution of things, and we have auroral columns streaming off from the regions about this pole, and darting off past the nucleus (like the matter of the zodiacal light from the polar regions of the sun). The attractive action of the pole of the nucleus, and of the lower portions of the coma, will tend to make the orbits of the columns more or less concave toward the nucleus. If we suppose the matter flowing away to consist of short columns, or of isolated particles, then the nucleus would have no sensible influence on the form of the orbit, unless we suppose the auroral matter to be subject to the law of gravitation. But the impulsive action of the auroral matter flowing from the sun will tend to produce an effect similar to that which would result from an attraction of the nucleus, and will exalt the tendency of the columns to flow past the nucleus. Either of the two causes which have been specified as tending to expel a portion of the cometic matter from the side of the comet that is turned toward the sun, acting singly, may perhaps be sufficient, under favorable circumstances, to develop a tail; and, if the line of the poles should materially deviate from coincidence with the radius vector of the comet, two tails might be formed by these two separate actions. Again, there may be separate layers of magnetic matter, as in the case of the sun, at different distances from the nucleus, with the lines of their poles non-coincident by reason of the unequal directive action of the sun upon them. Auroral columns may also dart forth from any point in the depth of the photosphere, and pass out through the outer line of the moving columns of the principal tail, under a considerable angle. The flow of such columns from the vicinity of the nucleus on the side toward the sun, in two opposite directions, was beautifully seen on one occasion during the last appearance of Halley's comet. A careful study of the phenomena here referred to, and of other peculiar phenomena presented by this comet, led Bessel to the conclusion, that the cometic matter was susceptible of polarity. It is to be observed that the columns which pass into the tail will tend to change their direction as they move on, so as to be continually parallel to the directive force of the nucleus, until, in passing the plane of the magnetic equator of the nucleus, they become nearly parallel to the line of the poles; after which they will continue on, forming probably a series of columns in a line with each other, and having their similar poles in proximity.

Such is a brief outline of the new views with regard to the intimate nature of the cometary bodies which have been suggested by the theory of magnetic photospheres. How far they may serve us in explaining the mysterious phenomena of these bodies, can only be made to appear by entering upon an elaborate research.

The opinion I have now advanced, that it is the same species of matter that is seen in the streamers of an aurora, the zodiacal light, and the coma and tail of a comet, is likely to be suggested to any mind by the similarity of their appearance. That it is, in each instance, some exceedingly subtile form of matter, is manifest from the fact that it has no power to absorb the light of the stars. This may certainly be predicated, with more probability, of electric ether, than of any other known form of matter, not only because it is more subtile, but because there are good and sufficient reasons for believing that the vibrations of electric ether are everywhere the primary exciting cause of light. It is well known that Becquerel and other distinguished French physicists maintain that the light of self-luminous bodies has its origin in the restoration of the electric equilibrium.

If any person should incline to the conception of some form of subtile magnetic matter rather than that which I have adopted, I have to say, that, though less probable in itself, the multifarious phenomena I have considered admit, for the most part, of satisfactory explanation by means of it.

The general views that have been presented in this and the previous paper involve certain ideas with regard to the nature of electricity, and of the relations subsisting between the two principles of electricity and magnetism, and lead quite naturally to a new general theory of electricity, which it may be well to state. Let it not be supposed, however, that this theory is essential to the development of the several representative theories that have been propounded. These, in fact, necessarily involve only the fundamental idea of bipolar particles of subtile matter. I conceive, then, that the molecules of bodies are surrounded by atmospheres (photospheres) of magnetic or electric ether, which are the abundant source of all electric and magnetic phenomena. The particles of these ethereal atmospheres are arranged in lines determined in position by the directive force of the central polarized molecule. Similar poles are, therefore, laterally contiguous, and tend to repel each other. Free electricity may be imagined to be the individual ethereal particles derived from these atmospheres, and held in position on the outer surface of bodies by their directive action; a systematic arrangement of their poles having been produced by excitation or induction. These particles, having their similar poles contiguous, would repel each other and tend to flow along the surface. On this view, positive electricity would be electric ether with the north pole turned outward, and negative electricity the same ether with the south pole turned outward. Another view more in accordance with the generally received theory of two fluids, and which furnishes a readier explanation of the details of the phenomena of excitation and induction, is, that the bipolar particles of ether are separated, by friction or inductive action, into two unipolar particles, and that these unipolar particles are the two electricities in their active state; that in which they exhibit tension.* This hypothesis is equivalent to the supposition that magnetic polarity is attributable to the union of the two electric fluids, or ethers; that is, that this union forms true magnetic ether, and implies, collaterally, that the inherent polarity of molecules is but the result of the union of two particles, or atoms, that have an attraction for each other. This, like every other hypothesis, must be tested by its ability to explain all the phenomena that fall within its scope. If it represents satisfactorily a wider range of phenomena than have hitherto been grouped under one conception, then it should be adopted as a valuable generalization.

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* A voltaic current I suppose to be the propagation of a polarization.

7. On the Swelling of Springs and the Reappearance of Streams just before Rain. By Professor John Brocklesby, of Trinity College, Hartford, Ct.

Last year I had the honor to present to the Association a short article "Upon the Rising of Water in Springs immediately before Rain." I was then able to adduce only two instances of this phenomenon; indeed, but very few persons had noticed it throughout the section of country where I reside, and the fact was either generally unknown or discredited. After the appearance of the article in the public journals, I received letters from a number of intelligent gentlemen, kindly favoring me with much information upon this point. Another paper upon the subject has therefore been prepared for the present occasion.

Mr. J. M. Pelton, of Warwick, Orange County, N. Y., thus writes: —

"I have just seen your article on the Rise of Water in Springs immediately before a Rain. The first notice of the matter I ever observed by a man of science. In this paper you state that you had requested information through the public papers, and had been rewarded with only one instance additional to that noticed by yourself.

"Had I seen this request, I might have communicated to you the here *well-known fact*, or at least *firmly settled belief*, that not only one or two springs rise before a rain, but that *all* do more or less, particularly in *dry weather*, the only time we are likely to notice it. So well is this known by persons of observation, that it would cause surprise to hear it doubted. In fact, it is considered as one of the surest signs of rain in a drought.

"The town of Warwick is skirted on the south by the Warwick Mountains, and down these lofty ranges flow many streams which are fed by springs. These streams, in many instances, dry up during the heat of summer, particularly in droughts, an unusual share of which we have had within the last ten years. The brooks, when dry, leave a bed of stones and gravel, and it is a common thing before a rain to see this gravel *far below* the then running water grow moist, with now and then a little pool standing between the stones where all was dry a few hours before. This is usually followed by a considerable lengthening of the stream, and then by rain. These phenomena I have frequently witnessed myself in two streams which flow through the farm on which I reside, and in other instances also."

Mr. Enoch H. McCall, of Windsor, Ct., gives the following facts: —

"In the county of New London and town of Lebanon, Ct., on the farm of my father, Henry McCall, is a spring that seldom fails, but in very dry times it ceases to flow. Whenever the fountain, after becoming dry, recommences running, the neighbors regard it as a sure sign of rain, — a sign that never disappoints their expectations."

In a letter received from Mr. Watson Newbold, who resides near Columbus in Burlington County, N. J., the writer thus remarks: —

"My object in addressing you is to say, that from my childhood (and I am now about fifty years old) I have been accustomed to hear the remark from old and observing men, 'that we shall have rain in a day or two to a certainty, because the small springs have risen, and sometimes dry ones started.' The phenomenon is believed to be unerring in a *very dry* time. I do not recollect observing it, or hearing the above remark, in a *wet* time."

A gentleman of the name of Dill, who lives in Newark, Ohio, writes as follows: —

"Through my garden, and forming the segment of a circle, flows a brook which has its source in the hills some five or six miles to the west. After it descends into the valley of the Licking, it passes first through a clayey alluvium, and then, near its entrance into the Licking River, through an alluvial gravel. Although it runs a mill some four miles to the west of my premises nearly the whole year round, the bed becomes dry in my grounds frequently during droughts. This brook is our *barometer*. When the water is flowing, we notice that it diminishes rapidly some twenty-four to thirty-six hours before a shower. Often the bed of the stream becomes quite dry, and some twelve to eighteen hours before the rain there is an unusual increase in the quantity of water flowing. In the last twelve years I have had four or five gardeners who have relied upon the phenomena of this brook as indications of the weather."

Still further: "It is well known in the West, that when the elements are preparing for a shower there is a rapid diminution of the water in our streams. The evaporating power of the atmosphere is probably at its maximum from thirty-six to forty-eight hours before the fall of rain, and at its minimum from twelve to eighteen hours previously. I have given you these facts,—which I have long observed, and which have attracted the notice of many of my acquaintances who are interested in the volume of water in our streams, such as millers,—and my mode of solving the problem."

From Mr. A. N. Haynes, of Philadelphia, much information was likewise received touching the phenomenon in question. In the course of his remarks, Mr. Haynes observes:—

"The statements of my father, a man of more than ordinary information and observation for a farmer, first drew my attention to this subject years ago, when living with him on a farm near Medford, in Burlington County, N. J. He often asserted that *water rose in springs just before a rain*, and prognosticated rain from this fact. And my impression is, that there exists in the neighborhood a sort of traditionary belief in this phenomenon; but this might possibly have its origin in the remarks of my father.

"In the summer of 1838, an excessively hot and dry summer, my attention was directed to a small rill, which rarely fails; but during this summer its bed was dry for some time. It flows by the side of the public road, at Cotoxia Bridge, having its sources in the springs at the foot of the hill-side along which it runs, emptying into the upper end of Haine's mill-pond. The extent of its course is not over two hundred yards, being from the brow of the hill to the edge of the creek. In the early part of September I noticed that this rill, the bed of which had been dry for a considerable time, had recommenced running, and the road was also moist in several places. This change occurred previously to any rain, of which none, or none of any consequence, had fallen since some time in June. Within a few days after this observation, there was a great storm of rain, which completely saturated the earth.

"I have observed similar circumstances in other localities of the neighborhood. On the 22d of July, 1853, riding into the country with my brother-in-law, in the latter part of the afternoon, I remarked in a locality similar to the one above described in detail, that the side of a bank which descended toward a creek that ran by the side of the road was moist to within two or three feet of the top, and that water appeared to be oozing out, and I think was running at the foot of the bank. It was quite dry at the time, and vegetation appeared to be suffering considerably from the drought. About ten miles to the eastward it was very dry; the pastures were parched up, and even the corn and the forest-trees had begun to show the effects of the continued heat.

"From these circumstances and the position of the bank I judged that it had been entirely dry, and that the reappearance of moisture on its sides, and the oozing out of water at its foot, now indicated an approaching rain. I expressed this belief to my companion, who did not concur with me, although to him as a farmer such an event was very desirable. The weather was hazy at this time, but it had been so to some extent for a week or so. The following morning it rained slowly enough to wet the ground to the depth of about half an inch."

In a newspaper entitled "The Republican," published at Savannah, I find an article under date of December 29th, 1853, affording corroborative testimony upon this point; indeed, the writer seems to think it strange that every one is not already acquainted with this phenomenon. He thus remarks: —

"It is well known to every observant person on Southern plantations, especially in the hill country of Georgia, that rain after a drought is preceded by the rising of water in the beds of exhausted streams. We have ourselves observed it in the northeastern portion of the State. Nay, more, the phenomenon was well known to Shakespeare three hundred years ago; for in the second act of Richard the Third he says:

> 'Before the days of change, still it is so:
> By a divine instinct men's minds mistrust
> Ensuing danger; as by proof we see
> The waters swell before a boisterous storm.'"

The facts in respect to the State of Georgia are important in reference to the subject in question, but the circumstance which Shakespeare mentions relates, I apprehend, to another but kindred phenomenon, to which I shall presently allude.

In the San Francisco Evening Journal, dated September 12th, 1853, are the following remarks, under the head of "The Annual Rising of the Springs": —

"Most of those persons who have been resident in this State during the alternate seasons have doubtless either observed or heard noticed the fact, that, immediately before the setting in of the rainy season, the small creeks and streams begin to rise and flow where naught but a dry water-course had lain unmoist for months. The phenomenon has been noted by many, and has elicited a variety of explanations."

In addition to the preceding statements, I have received from other sources information of a like import, which it is unnecessary to detail after the facts already cited.

From the testimony adduced we are warranted in making the following inferences: —

First. *That moisture and water reappear in the exhausted and dry beds of streams immediately before a rain;* and

Secondly. *That springs swell and rise under the like circumstances.*

The rising of springs before a rain I have attributed in a previous paper to a diminution of atmospheric pressure, and shall presently speak of this phenomenon again, after considering the first division of the subject, namely, the reappearance of moisture and water in the dry beds of brooks previously to a rain.

I. Whenever, during a time of drought, a brook gradually dries up, the lower end of the bed usually becomes first exhausted, the water retreating by degrees towards the head of the stream. Here, even when the hot weather has continued long, a scanty rill may perhaps be found. Its shallow waters are exposed to the full blaze of the sun, and, as they advance slowly along the almost exhausted bed, the liquid thread becomes more and more attenuated. At last it ceases to *flow*, but for some distance below the point where it stops little pools still remain in the hollows of the water-course. At a still lower point these disappear, and are succeeded by moist stones and gravel, and beyond these the bed of the brook is dry on the surface, but moist perhaps a few inches below.

This gradual diminution in the quantity of water as we follow down the bed of the stream is evidently due, under a *mean atmospheric pressure*, to the intense evaporation that prevails beneath the fierce rays of a summer's sun, undimmed for many days, and perhaps weeks.

This process of evaporation operates in two ways: —

First. By diminishing the volume of water discharged by the springs that form the brook. For these springs derive more or less of their water from the adjacent lands which they serve to drain; but these lands, parched by the heats of summer, no longer furnish their usual supplies. They have gradually withdrawn them, and the springs now discharge a smaller stream.

Secondly. The diminished volume of water that issues from the spring wastes rapidly beneath the hot and dry atmosphere, as it flows through a long and exposed channel, until at length the evaporation is equal to the flow.

Then the stream stops, and, the drought continuing, the brook shrinks gradually back towards its fountains, leaving little pools in the depressions of the water-course, which in time are also exhausted, until at length a portion of the bed of the brook, and possibly all, becomes bare and dry.

When a brook is in this condition, it appears, from the facts adduced, that immediately before a rain the springs of the brook rise, the stream starts where there were only pools before, pools form in the wet gravel, and the upper part of the dry water-course becomes moist. And that in some cases the stream swells over these intermediate spots, and flows far down over the bed that was before dry.

Two causes conspire, in my opinion, to produce these results in the extra-tropical regions: —

First. *A diminution of the atmospheric pressure upon the jet of each spring*, in consequence of which its flow is increased, and a greater quantity of water discharged into the channel of the brook in a given time than before this diminution occurred.

Secondly. *A less energetic evaporation.* For when a storm is gathering, the atmosphere becomes gradually more and more charged with vapor. It is not, so to speak, as thirsty as it was before; the process of evaporation then goes on more slowly, and the brook throughout its winding length loses less water by evaporation than it did when the air was hot and dry. Especially is this the case when the sky is overcast, and the burning rays of the sun cease to fall upon its waters.

It may be objected, that the starting of the springs is due to the diminution of evaporation throughout the regions in the vicinity of the springs, and not to the decrease of atmospheric pressure.

This position is untenable, for it is a well-known fact, that, when the springs are low in consequence of a drought, *long-continued rains* must drench the adjacent lands before the moisture that percolates the soil reaches the sources of the springs and swells their waters. A decrease of evaporation from the surface of the soil, for the space of twenty-four or forty-eight hours, could not in the least affect the sources of the springs.

That the decrease in evaporation throughout the long line of the water-course must aid in producing the phenomenon under discussion, admits of no doubt. For the difference in the amount of water evaporated from a given surface beneath a dry, hot, and serene atmosphere, and under one which is nearly filled with moisture, and perhaps obscured, is by no means slight. In the low latitudes the variation that exists between the evaporating power of the atmosphere in the day and in the night produces a phenomenon similar to the one we are now considering.

In a letter received from a correspondent in California, under date of November 20th, 1853, the writer, Mr. A. S. Easton, remarks as follows: —

"This season, while engaged in surveying, I have observed that small streams were *higher in the morning than in the evening*, during the dry season. I have also been informed by gentlemen who have crossed the plains of California, that, in places where there was no water to be obtained in the *evening*, for themselves or their horses, in the *morning* there would be a sufficiency found for the entire party. I account for this phenomenon by the fact that the evaporation is much greater during the day than in the night. I have noticed the thermometer standing as high as 150° Fahrenheit in the sun."

The same writer still further remarks: — "I have also heard it affirmed here, as well as at the East, that a water-mill will saw or grind more during the same number of hours in the night than in the daytime. The cause of this I think must also be attributed to a variation in evaporation. The mill performs more work during a given time in the night than in the day, simply because there is then more water-power."

In countries intersected by snow-capped mountain ranges, and situated in the low latitudes where the rainy and dry seasons prevail, the rise of water in brooks before the setting in of the rainy season receives an additional solution. In respect to California, my correspondent, Mr. Easton, thus observes: —

"Snow is seen on the Sierra Nevada Mountains during the entire year. When the wet season commences, the rains pour down in the interior on the Sierra Nevadas before they occur in the valleys beneath. The mountain streams are consequently swollen throughout their entire length down to the valleys, a few days before the rains reach the latter. The Sacramento papers of last week notice a rise of two feet in the Sacramento River several days before it commenced raining there.

"Nelson Creek takes its rise in the snow-banks of the Sierra Nevadas, and flows with great rapidity into Feather River. This creek rises so suddenly, that, when it is even six or eight feet higher than its usual level, the rise is not perceived in the Sacramento, and scarcely in Feather River. At Valparaiso, where we discover the same general outline of formation as in California, we find also this phenomenon of the annual rising of the waters of streams before the occurrence of rain at the place of the rising."

II. We now pass to the second division of the subject, namely, *the rising of the waters of springs*, in distinction from brooks, just previously to a rain. And we shall speak of this phenomenon as it exists in the temperate climes, and uninfluenced by the proximity of snow-capped Alpine ranges.

That fountains thus swell is undoubtedly true, but how is the fact to be accounted for? We must exclude in this case the cause peculiar to tropical climates, and we have also shown that the diminution of evaporation from the adjacent lands before rain is totally insufficient to account for the circumstance. It might be alleged that the evaporation from the exposed surface of the spring itself is *less* just before a rain than for some days previous, and it therefore rises at this particular time. But this objection is annihilated by the fact, that the Rutland Spring, where this phenomenon is very marked, is always *covered over with boards, being never subjected to the blaze of a summer sun.* I therefore fall back upon the fact which I have before assigned in explanation of this phenomenon, namely, *a diminution in the atmospheric pressure* before a rain; which, in my opinion, is the cause of this apparent anomaly.

Cases analogous to this exist in nature.

Kaemtz remarks, that "in many coal-pits hydrogen gas is so abundant in certain galleries, that they are obliged to be abandoned, but that more frequently it is developed in holes and caverns, communicating with the gallery by narrow fissures. While the gas is in small quantities its presence is not indicated, but there is frequently a detonation at the time of a storm. This coincidence is easily explained. The storm is, indeed, generally preceded by a sudden fall in the barometer; the pressure becoming less, the hydrogen escapes from the fissures, and, mixing with the air, fills the gallery; and if any one is so unfortunate as to enter with a light, an explosion instantly occurs."

The same author observes, "that a similar phenomenon occurs in springs containing carbonic acid; that these waters are probably charged with this gas in cavities where the atmospheric pressure is much greater than on the surface of the earth; but when the spring arrives at the surface, the gas is liberated in the form of bubbles, especially if the barometer is low."

The swelling of the sea in advance of a storm, to which Shakespeare doubtless alludes in the lines above quoted, and *not to the rising of springs*, is also a case in point. According to the most reliable observations, the ocean hurricanes are preceded by a heavy swell, or *storm-wave*, which is, in part, accounted for by the diminished atmospheric pressure that attends them. In the centre of the hurricane the mercury of the barometer falls to the extent of from one to two, and even two and a half inches. On this account the atmospheric pressure upon the ocean at a distance where the barometer stands at its ordinary level, will cause the sea to rise in the vortex of the hurricane two feet or more above its usual height. And throughout the whole area of the storm the waters will be thus elevated in proportion to the diminished atmospheric pressure, — in the same way as any liquid rises in a pump when the air is rarefied within. The vast volume of water which forms the storm-wave is driven along with the tempest and before it; the sea swelling in advance of that portion of the storm-area where the tempest rages in its might.

In conclusion, I would remark, that the additional facts now obtained in regard to the rising of springs before a rain, coming as they do from localities so widely separated, all tend to prove that this phenomenon *is general*, which must be the case if its cause is the diminution of the pressure of the atmosphere.

THE following papers were read, but a copy has not been furnished for publication: —

8. ON THE TRANSPARENCY OF THE OCEAN. By COMMANDER JAMES GLYNN, U. S. N.
9. ON THE GULF-STREAM. By LIEUTENANT M. F. MAURY, U. S. N.
10. ON THE BASIN OF THE ATLANTIC. By LIEUTENANT M. F. MAURY.
11. ON THE MAGNETIC FORCES OBSERVED ALONG THE LINE OF THE BOUNDARY BETWEEN THE UNITED STATES AND MEXICO. By MAJOR W. H. EMORY, U. S. A.
12. EARTHQUAKES OF CHILI. By LIEUTENANT J. M. GILLISS, U. S. A.

IV. METEOROLOGY.

1. ON THE PROBABLE INCREASE IN THE NUMBER OF HAILSTONES OCCURRING IN CUBA, ESPECIALLY FROM 1844 TO 1854. By ANDRES POEY, of Havana.

THIS is an important question in the study of Cuban climatology, and of the meteorological agents which rule the intertropical regions of the Columbian Archipelago; but the obtaining of such a determination of the fact in question will be impracticable, if a comparable system of meteorological observations, taken from the principal and more advantageous latitudes of the islands of the Antilles, is not organized. Such an enterprise can only be undertaken by the respective governments of each colony, and it is hoped that the proposed organization of the meteorological system, both by sea and land, will embrace the islands of the Antilles.

If the cause of hail seems to be yet unknown, and meteorologists variously explain it, some by an electric and others by an aqueous action, and with imperfect knowledge of the atmospheric circumstances under which this meteor is produced, — if all this is unknown, I repeat, how much greater must be our ignorance respecting the non-prevalence of hailstones in certain islands of the Columbian Archipelago, and the complete absence of them in other islands.

In the past century, and in the beginning of the present one, hail has been always considered as an extraordinary phenomenon in those islands, as Jamaica and Martinique, and in the city of Caracas, where hail fell in very distant epochs. Chavalon[*] affirms that a hail-storm which occured in 1721 in the island of Martinique excited the astonishment of the inhabitants. Sloan[†] considers as a remarkable phenomenon a hail-storm which occurred in Jamaica on the 9th of September, 1688. And Long[‡] expresses himself in the same terms respecting another one which took place in the same island in 1757. Mr. John P. Know,[§] a later writer, says that, "during the period over which our observations have extended (from 1843 to 1851), we have known hail to fall but once in the West Indies. It occurred at St. Croix, 13th April, 1844."

As it is not my intention to consider in detail the hypotheses respecting the real or apparent increase of hail in the Antilles, I shall present only the deductions which have been suggested to me in the analysis of the materials I have been able to collect on this subject, and shall only treat here of hail with relation to Cuba.

TABLE I. — *Showing Thirty-five Cases of Hail falling in the Island of Cuba in a Period of Seventy Years, from* 1784 *to* 1854.

LOCALITY.	Year.	Month.	Day.	Hour.
Havana,	1784	March	8	
Havana,	1825	March	3	P. M.
Havana,	1828	April	29	P. M.
Isle of Pinos,	1835			
St. Jago de Cuba,	1844	April		
St. John de los Remedios,	1845	April	8	3½ P. M.
St. Jago de las Vegas,	1845	April	27	
Havana,	1846	February	14	
Vuelta de Arriba,	1846	October		
Villa Clara,	1846			
Alacranes,	1847	February	21	4 P. M.
Havana,	1847	April	21	1 to 2 P. M.
Havana,	1848	June	14	2 P. M.
Cardenas,	1849	March	25	
Havana,	1849	March		
Simones Grandes,	1849	March		
Sagua Oriental,	1849	April	1	P. M.
Seibado (V. C.),	1849	May	25	

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[*] Voyage à la Martinique, (Paris, 1763,) p. 135.

[†] Natural History of Jamaica, (London, 1707,) Vol. I.

[‡] History of Jamaica, (London, 1774,) Vol. III. p. 652.

[§] A Historical Account of St. Thomas, (New York, 1852,) p. 191.

LOCALITY.	Year.	Month.	Day.	Hour.
Villa Clara, ...	1849	August		
St. Antonio de los Baños, .	1849	August	9	1 to 2 P. M.
Havana, ...	1849	August	27	
Havana, ...	1849	August	29	P. M.
Sabanilla, ...	1850	June	22	5 P. M.
Havana, ...	1851	March	30	1½ to 2½ P.M.
Sabanilla, ...	1852	May ?		
Havana, ...	1853	March		
Sabanilla, ...	1853	March		
Melena del Sud, ...	1853	May	1	4½ P. M.
Moron, ...	1853	June	22	2 P. M.
St. Jago de Cuba, 5 leagues north, ...	1853	June	22	
St. Jago de Cuba, 1½ leagues north, ...	1853	June	22	
St. Jago de Cuba, . . .	1853	August	21	3½ P. M.
Madruga, ...	1853	December	31	At night.
Villa Clara, ...		April	25	3 to 4.
Havana, ...	1854	April	4	4½ A. M.

The first author who, to my knowledge, has mentioned the hailstorms occurring in Cuba, confining himself to Havana, is Baron Humboldt, who affirmed they took place *every fifteen or twenty years.* If we stop to consider the reason which Humboldt could have had for fixing so long a period, we shall see that he probably founded it on the data possessed by him at that time.

Humboldt visited the island of Cuba for the first time on the 19th of December, 1800, and remained there till the 16th of March, 1801; and returning to Europe, he stopped again in Cuba in January of 1804, remaining there two months. As it seems, Humboldt made, for the first time, some reference to instances of hail falling in the equatorial region in the work he commenced publishing in folio in 1814, under the title of *Voyages aux Regions Equinoxiales du Nouveau Continent*, etc., Vol. I. p. 586, and Vol. II. p. 272; but there he does not refer to any case which occurred in Cuba. But it is in his Political Essays on the Island of Cuba, published in 1826, Humboldt says that "Hail only happens at Havana every fifteen or twenty years." * All these particulars must be borne in mind in examining the opinion of this wise man.

It may be asked, what reason Humboldt had for fixing a period of fifteen to twenty years. Can it be that during so long a period no hail fell really in Havana, or was there no record of it? I am not furnished with data to answer the first question, but should suppose that Humboldt would have made as many investigations as possible from the rusty records, especially in reference to the periodicity. To answer the second question, it suffices to consider the indication which I farther do about the years in which hail fell at Havana, specifying the intervals in which it did not happen; and it will be seen that the first case of hail occurred during the tempest of the 8th of March, 1784, the same to which Humboldt may have adverted, and afterwards until the 3d of March, 1825, no other appears within an interval of forty years. Therefore, Humboldt, having arrived in Cuba for the first time in 1800, may have been informed of the cases of hail-storms that occurred before this year, and the most remote which he found was perhaps that of 1784, sixteen years before his arrival in Cuba. I do not affirm that such was the reasoning of Humboldt, but it is very probable that it was so, unless there were records of hail falling from 1784 to 1800, or from 1800 to 1825; that is, sixteen years before his arrival in Cuba, or twenty-five years between his departure and the publication of his Political Essays.

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* Essai Politique sur l'Ile de Cuba, (Paris, 1826,) Vol. I. pp. 79 and 90.

Mr. José Zacarias del Valle,* in 1849, called attention to the opinion of Humboldt, and argued that either hail-storms were not formerly repeated so often in Havana, or that the observations referred only to the capital; and that in the last case the greater frequency of hail in the years which have elapsed since the last hurricane was proved respecting ancient epochs, from the fact that the meteor was then noted in the almanacs as a prodigy.

Though Mr. Valle at that time had only notice of the hail-storms of 1847, 1848, and 1849, which occurred in Havana, and could not prove the absolutely greater frequency of hail for want of data, such as were after that furnished, he at least suspected it for that city as well as for the interior of the island.

I will first take into consideration the hail-storms which occurred in the city of Havana, and give the following table.

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* Lecciones Elementales de Meteorologia, (Havana, 1849,) pp. 126 and 128.

TABLE II. — *Hail-Storms occurring in Havana from* 1784 *to* 1854.

March 8th, 1784.
40 years of interval.
March 3d, 1825.
2 years of interval.
April 29th, 1828.
17 years of interval.
February 14th, 1846.
April 21st, 1847.
June 14th, 1848.
March, 1849.
August 27th, 1849.
August 29th, 1849.
1 year of interval.
March 30th, 1851.
1 year of interval.
March, 1853.
April 4th, 1854.

This table shows that the first hail-storm which I have notice of as having occurred in Havana is that of the 8th of March, 1784. This hail descended during the tempest of St. Juan de Dios, and Mr. Desiderio Herrera* affirms it is the most ancient known to have fallen in that city. From 1784 to 1825 no hail appears for a lapse of forty years. From 1828 to 1846, seventeen years appear without hail; but from 1846 to 1849 there are four consecutive years in which the meteor took place, with the particularity of there having been three cases in 1849, one in March and two in August. The year 1850 presents no hail, and neither does 1852; but there was hail in 1851, 1853, and 1854.

In regard to the epochs in which hail has fallen, and falls now, no law can be deduced from the single cases presented in the table; for, as I said before, it would be necessary to be sure of no other having occurred in the seventy years which have elapsed from 1784 to 1854; but unhappily the study of meteorology in Cuba is in its infancy, or rather is not yet born, for we have neither an observatory nor a person who could employ himself in observing regularly, and at fixed hours, different instruments.

If, by want of data, no law can be deduced respecting the periods of hail-storms in Havana, I may at least have proved the greater abundance of hail in these last years, and principally after the hurricane of 1846. And such being the case, may not the same hurricane have influence on the more frequent occurrence of the meteor?

These are questions which I only propose for future consideration, when a greater collection of data will put it in our power to answer them in a decisive manner.

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* Memorias sobre los Huracanes en la Ysla de Cuba, (Habana, 1847,) p. 47.

TABLE III.—*Monthly Distribution of Hail-Storms at Havana, with the Average Temperature for each Month.**

Month.	Cases.	Temperature.	Month.	Cases.	Temperature.	Month.	Cases.	Temperature.
		Cent. °			Cent. °			Cent. °
January,	0	21.87	May,	0	25.54	September	0	26.87
February,	1	23.35	June,	1	27.22	October,	0	26.03
March,	5	23.37	July,	0	27.47	November,	0	23.96
April,	3	24.79	August,	2	27.54	December,	0	22.65

Mr. Ramon de la Sagra, in his first work on the Climate of Cuba,† says, that hail is formed by the change of the humid and warm winds of the south to the dry and cold of the north, which, according to him, happen in February and March; and he afterwards quotes the hail of March, 1825, which he saw. But in his second work, published in 1842,‡ he affirms that the times he saw hail in Havana were always in the *warmest months* of the year; and it further appears, that those times when he saw hail fall are reduced to the 3d of March, 1825, already quoted from his work of 1827, and to the 19th of April, 1828.

An opposite condition in this respect appears from these two observations; as on one side hail occurs in the cold months of February and March, and on the other in the warmest ones of the year. But as Mr. Sagra had only knowledge of those two cases which took place in March, 1825, and in April, 1828, his opinions are scarcely important.

It was indispensable for me to take into consideration the opinion of Mr. Sagra, on account of his being the only author, to my knowledge, who has pointed out the monthly distribution of hail at Havana; and also, as I said before, because the months in which the two hail-storms of which Mr. Sagra had knowledge contradict his theoretical conclusion; and so do the eleven cases that took place at Havana, according to my Table III.

It is then proved for the twelve cases of hail-storms which my table presents, first, that the greatest frequency takes place in March; second, that April, the average temperature of which comes near to the annual mean, gives three cases, and August, which is the warmest in the year, two; from which it results that the maximum does not fall, as Mr. Sagra affirmed, in the warmest months of the year.

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* This temperature has been deduced by Mr. Ramon de la Sagra from seven years of observations which he made at Havana.

† Memorias para servir de Introduction á la Horticultura Cubana, (New York, 1827,) p. 13.

‡ Historia Física, Politica, y Natural de la Ysla de Cuba, (Clima, Paris, 1842,) p. 117.

As to the circumstances which accompany the hail-storms, Humboldt was the first to indicate that this meteor is only observed during the electric explosions and the S. S. W. winds.*

Hail-Storms in the Interior of the Island.

Having presented the instances of hail-storms which, to my knowledge, have occurred in Havana, and the theoretical consequences I have been able to deduce in relation to their annual, monthly, and hourly distribution, and other details, I pass now to those observed in different localities of the island, including those of Havana, confining myself, on account of the small number, to general deductions.

TABLE IV.—*Annual Distribution of the Hail-Storms occurring in all the Island from* 1784 *to* 1854.

1784.
40 years of interval.
1825.
2 years of interval.
1828.
6 years of interval.
1835 (Island of Pinos).
8 years of interval.
1844.
1845.
1846.
1847.
1848.
1849.
1850.
1851.
1852.
1853.
1854.

By this table we see that from 1784 to 1825 forty years elapsed without a single case of hail; that from 1825 to 1828 there were two years without it; from 1828 to 1845, fourteen years, for the hail of 1835 took place in the Island of Pinos; from 1844 to 1854, hail fell annually; although it is not proved that in these intervals of forty and fourteen years there was really no hail.

TABLE V.—*Number of Hail-Storms occurring annually in the Island from* 1784 *to* 1854.

Year.	Cases.	Year.	Cases.	Year.	Cases.
1784	1	1846	3	1852	1
1825	1	1847	2	1853	8
1828	1	1848	1	1854	1
1835	1	1849	9		
1844	1	1850	1		
1845	2	1851	1		

One case without year. Total, 35.

This table shows that the greatest abundance of hail occurred in 1849.

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* Essai Politique sur l'Ile de Cuba, Vol. I. p. 90.

in which year there were nine cases, and after, in 1853, in which eight cases took place; in 1846, there were three, and in 1845 and 1847 only two cases. In the other years there was only one case in each.

If, on one side, are borne in mind the intervals of forty and fourteen years in which no hail appeared, and on the other side the consecutive cases from 1844 to 1854, and, above all, the nine cases of 1849, and the eight of 1853, all these considerations seem to indicate a real increase in the number of hail-storms, at least from 1844.

TABLE VI. — *Monthly Distribution and Number of Hail-Storms occurring in the whole Island from* 1784 *to* 1854.

Month.	Cases.	Month.	Cases.	Month.	Cases.
January	0	June	5	November	0
February	2	July	0	December	1
March	8	August	5	Three cases without month. Total, 35.	
April	8	September	0		
May	2	October	1		

It may be observed upon this table, first, that the thirty-five cases of hail which appear there are distributed through all the months of the year, excepting January, July, September, and November; second, that the months of March and April, which represent the average temperature of the year, are the most abundant in hail, and next come June and August, which are the hottest months in the year; third, that July, which presents a middle temperature between June and August, has no hail out of the thirty-five cases which correspond to a lapse of seventy years, this being a very remarkable fact, and worthy of further investigations to prove the phenomenon; fourth, that March and April show the maximum of hail for the cases which occurred in the island, as well as for the cases of Havana, mentioned before; and that this circumstance proves what I have said before, namely, that the hail did not fall, as Mr. Sagra supposed, in the hottest months of the year.

TABLE VII. — *Geographical Distribution of Hail-Storms by Departments and Localities.*

Occidental Department.		Melena del Sud,	1 case.
Havana,	12 cases.	Madruga,	1
St. Antonio de los Baños,	1	Alacranes,	1
Santiago de las Vegas,	1	La Sabanilla,	3
Limones Grandes,	1	Cardenes,	1
Vuelta Arriba,	1		—
Island of Pinos,	1	Total,	24 cases.

CENTRAL DEPARTMENT.	
Sagua Oriental,	1 case.
Villa Clara,	2
District of Potrerillo (Villa Clara),	1
District of Seibado (Villa Clara),	1
St. Juan de los Remedios,	1
Moron,	1
Total,	7 cases.

ORIENTAL DEPARTMENT.	
District of St. Maria,	1 case.
District de la Enramada,	2
District de St. Andres,	1
Total,	4 cases.

It is seen by this table, first, that the Occidental Department presents twenty-four cases, the Central seven, and the Oriental four; second, that the hail-storms extend in the Occidental Department through a surface comprised between Havana in the north, Alacranes in the south, St. Antonia Abad de los Baños in the west, and Cardenas in the east; third, that hail-storms are distributed in the Central Department between Eastern Sagua in the north, Villa Clara in the south and west, and Moron in the east; fourth, that in the Oriental Department they are all concentrated in the jurisdiction of St. Jago de Cuba.

So that the most remarkable places in which no hail appears are, first, from St. Antonio Abad de los Baños to the Cape of St. Antonio; second, in the large extension of the jurisdiction of Puerto Principe; third, in the Western Department, excepting the jurisdiction of St. Jago de Cuba; fourth, in Matanzas, with the particularity of having taken place in localities which are at a very short distance from this city, that is, La Sabanilla and Cardenas. On the contrary, if any case had occurred in Guines it would be a phenomenon worthy of calling the attention of meteorologists; for this city is situated in the centre of a valley in which place hail never falls in the torrid zone.

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2. AN ACCOUNT OF A REMARKABLE LUNAR PHENOMENON WHICH WAS OBSERVED AT AUBURN, N. Y., ON THE EVENING OF FEBRUARY 16, 1843. By BLANCHARD FOSGATE, of Auburn, N. Y.

THE sky was of a light hazy blue, though uneven in the intensity of its color, but in the northeast the clouds were dark and heavy. There were about as many stars visible, and they were located, as near as may be, as seen on the diagram.* The lunar ring was very large and rather pale, but perfectly developed, and was to be seen some time before and after the appearance of the peculiar phenomenon. The spectra, which appeared on two opposite points of the ring, occupied the inner half of the ellipses, and were very brilliant, particularly the one on the centre of the cross situated to the south of the moon. This ellipsis was most perfectly formed, the outer portion being white, while that on the other side was in its outer portion very dark, and not so well defined, — its upper and lower extremities being drawn out and lost in the prevailing blue. The spectra were visible for about three quarters of an hour. The phenomenon was in perfection at thirty minutes past nine o'clock, at which time, as if by magic, two crosses of dazzling whiteness flashed into existence. Their geometrical form was so perfectly defined, and their proportions were so vast, that the spectator could not but be struck with awe at beholding them. One of these lines of light bisected the ring in its perpendicular diameter, and the other passed over it on its southern side, both reaching from a little above the horizon to about seventy degrees of altitude, while the transverse arms of both crosses, each being one half the length of the perpendicular, extended in a horizontal direction to an equal extent. This portion of the phenomenon, which was of five minutes' duration, was both preceded and followed by the spectra. During the continuance of this phenomenon, which made its appearance in the east just above the horizon, the wind was blowing lightly from the southwest; the mercury in the barometer stood at twenty-nine, and was rising; the thermometer indicated four degrees above zero of Fahrenheit, and the ground was covered with snow. Its whole duration did not exceed one hour and a half.

3. THE BRANDON TORNADO OF JANUARY 20TH, 1854. By PROFESSOR O. N. STODDARD, Miami University, Ohio.

ON the 20th of January, a storm of unusual violence swept over the State of Ohio from southwest to northeast. The same general current from the southwest was observed twenty-seven miles northeast of Little

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* Exhibited at the meeting.

Rock, Arkansas, on the 19th, followed by a clear, cold atmosphere on the 20th. The western part of Pennsylvania was also visited on the 20th by the same storm-current. The whole length of the observed track is more than eight hundred miles. Its breadth has not been ascertained. At Dubuque, Iowa, on the day the storm was traversing Ohio, the atmosphere was clear and very cold, with the wind from the northwest.

A high temperature prevailed in this southwest current; the thermometer standing at noon on the 20th, in Cincinnati, at 70°, and at 68° at Oxford, and the same at New Albany, Ia. The air was saturated with vapor, and the walls of brick buildings were dripping with moisture. Along the whole length of this track in Ohio, and as far as known in Indiana and Pennsylvania, there were violent rains, vivid lightning, and in some places a fall of large hailstones. At Xenia the storm began at about 11 A. M.; at Oxford, at 12½ M.; at Cincinnati, at 2 P. M.; and at New Albany, Ia., at the same hour. At this period a tornado of unusual violence was progressing through Knox County, in the northeastern part of the State. It first struck the earth in the southwest portion of Miller township, and thence moved in a direction N. 54¼° E. About two miles from the place of beginning, the writer commenced an examination of the path.

A survey of the track was made at this place, commencing on the right and terminating at Dr. Baxter's barn (I). (See Diagram, Section I.)

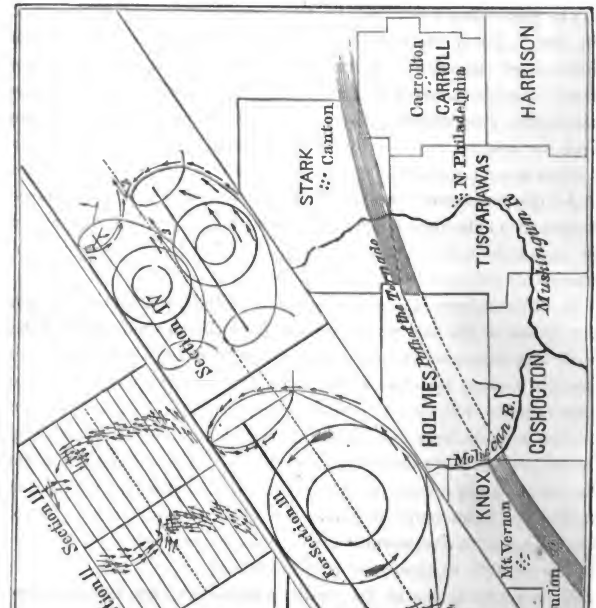
Course of the Storm, N. 56¼° E.

		°			°
1	Trees.	N. 56¼ E.	9	Across 8.	N. 56¼ E.
2	"	N. 45 E.	10	A fence.	N. 6 W.
3	"	N. 56¼ E.	11	An elm.	N. 11¼ W.
4	"	N. 22½ E.	12	A hickory.*	N. 45 W.
5	Several trees.	N. 45 E.	13	Three oaks.†	N. 45 E.
6	A tree.	N. 22¼ E.	14	Trees in orchard.	E. 10 S.
7	"	N.	15	Roof of barn.‡	E. 45 S.
8	Across 7.	N. 22½ E.			

* The top and half the trunk of this was taken and swept round *after* falling, N. 45° E. (See a broken arrow.)

† These stood near the hickory, and were prostrated when the hickory was turned round.

‡ The fragments were strewed in a long line for sixty rods, gradually curving round into the course of the storm.


STARK
Canton
Carrollton
CARROLL
N. Philadelphia
HARRISON
TUSCARAWAS
Muskingum R.
Path of the Tornado
HOLMES
COSHOCTON
Mohican R.
KNOX
Mt. Vernon
Brandon
Section I
Section II
Section III
Section IV
For Section II
For Section III
Fence
1
22
21

The plot of this survey will be found on the left part of Section I. Figures 2 and 3 represent E. Coleman's house and barn, both of which were unroofed. A rafter from the barn struck the house, passed through it, and into a chest standing within. Portions of the roof followed the course of the storm; others were scattered north and north by west.

From some peculiarities exhibited in this part of the path, it is supposed that the storm assumed here the form of a double whirl (see Section IV.); the right having the largest diameter, and in advance of the one on the left. This conclusion is based upon the following considerations: —

1st. The hickory (23), about seventy feet high, was prostrated into and across a shallow ravine N. 45° W.; the top and part of the trunk was afterwards turned round N. 45° E. Three small oaks near the hickory also lay N. 45° E. These had not been disturbed after falling; they must, therefore, have escaped the blast which prostrated the hickory, but yielded to that which turned it round. By referring to the left-hand cycloid, Section IV., it will be seen that objects would naturally be prostrated by it, at that point, in a direction N. 45° E. Some may be disposed to attribute these prostrations towards the east to the storm's axis. This explanation, though applicable here, fails when applied to the next case.

2d. A fence, forty rods long, near Baxter's barn, was thrown in opposite directions, one half southeasterly, and the other northwesterly. That there must have been a secondary whirl, which passed over this fence, seems evident; for had the rails been taken off by hand, and carefully laid for one half of its length on one side, and for the other half on the opposite side, the arrangement could not have been more distinctly marked.

3d. The direction in which Dr. Baxter's barn was struck, the gable end being driven in on the north, the east half of the roof taken off, and the fragments scattered in the line indicated by the curved arrows (Section IV.), conforms entirely to the reverse action of the secondary whirl.

No satisfactory reason can be assigned for this double whirl. About one fourth of the path on the right was in a dense forest, the rest being in cultivated fields. The obstructions thus offered may have disturbed its symmetry, and broken up the single whirl into two. Its existence, if admitted here, did not continue long; for one mile and a half east of this point, where the tornado again struck the forest, no trace of this irregularity can be found.

From this place the storm descended a gentle slope into the valley of Sycamore Creek. Rising on the opposite side, it entered Brandon, and first struck the house and barn of Dr. Wheaton (4 and 5). They fell as if crushed by an avalanche. The lower timbers were slid from the wall several feet towards the northwest; the foundation on the south was driven into the cellar; fragments of the siding were found five miles distant; a heavy plane was carried one fourth, and a handsaw one half of a mile.

A brick church (6) was utterly demolished; a brick school-house (7) shared the same fate; a log-house (8) was thrown down; the roof and ceiling of a small house (11) was raised, but not taken off; a Presbyterian church (9) was unroofed; a stable (14) was removed ten feet; a blacksmith and wagon shops (15 and 16) were prostrated; and E. Squire's brick house and out-houses (17) were swept away. Several other buildings in the eastern part of the town, which is not represented in the diagram, were unroofed or otherwise injured.

We will examine the bearings obtained in town. No observations were made here on the right of the track; the approach of night prevented it, and the writer was unable to return on a subsequent day to complete it.

For the sake of filling out the table below, so as to include in it the whole breadth of the storm, the first four bearings are interpolated from the survey made one fourth of a mile east of Brandon in the forest.

The buildings (7 and 8) give a mean bearing of N. 5½° E. The church (6), trees in an orchard (18), an out-house (10), and a bier carried north along the line of the dotted arrow, give a mean of N. 11¼° W. The church (9) is omitted; the roof was torn to fragments and scattered between northeast and northwest. Though the direction of the wind at this point could not be accurately made out from the fragments of the roof, yet other and more reliable indications were not wanting. The gable end on the south was crushed in, and the bier was taken from the graveyard, carried north across the street, and set down in the churchyard. Dr. Wheaton's house and barn (4 and 5) give a bearing of N. 22° W. A black oak a little west of Dr. Wheaton's was prostrated N. 33¾° W. In the valley of the creek two trees (near 21) give a bearing N. 39½° W. At this place a large limb was broken from a tree and carried S. 10° E. The following gives a tabular view of these bearings, the first four being taken from Section II.

Course of Storm N. 56¼° E.

1	N. 56¼° E.	These four were obtained one fourth of a mile east of the town, and belong to the right-hand portion of the path.
2	N. 45° E.	
3	N. 34° E.	
4	N. 8° 53′ E.	
5	N. 5½° E.	
6	N. 11¼° W.	
7	N. 33¾° W.	
8	N. 39½° W.	
9	S. 10° E.	

The bearings in this table are not a little remarkable for the regularity with which objects are curved gradually inwards as you pass from the right to the left side of the storm's path. The first coincides with the course of the storm; the eighth makes an angle with it of 95¾°.

One fourth of a mile east of Brandon, the tornado struck the forest. The place was considered favorable for determining the nature of its action. A survey was accordingly made across the track from right to left. The distances between the bearings were determined as nearly as possible by the eye; it being impossible to use a chain among such a mass of fallen timber.

The path of greatest violence is included between the dotted line and the right border of the storm. Within this space almost every tree was prostrated or broken. At the left of this line the prostrations were much less numerous, but sufficient to determine the direction of the wind. The arrows represent the direction of the trees, but not in all cases their relative distances from each other. Sometimes the trees were closely packed, at others separated by brief intervals. An arrow with a cross-bar indicates that the tree lay above another. The prostrations appear to have been made almost entirely by the front of the tornado, with the exception of the left-hand portion of the path, where the reverse action was in some instances the most violent. This front action of the storm divested its effects of much of that complexity which is sometimes observed where the action of different parts is mingled together.

Objects thrown down at the first stroke of the wind would truly represent a tangent to the curve at that point, whatever the nature of the curve might be.

As the trees which fell above others must have been thrown down after a part of the storm had passed, and therefore cannot indicate the front action, they are, on this account, either rejected in obtaining the bearings, or included with others in the reverse action. The parallel lines were drawn to aid the eye in grouping the trees; those lying on the lines are included in that interval whose other bearings most nearly correspond.

A Tabular View of Section II. representing a Survey ¼ Mile East of Brandon. Breadth of Track, ¾ Mile. Course of Storm, N. 56¼° E.

1st Group.	N. 45° E.	7th Group.	E. 34° S.
2d "	N. 34° E.	8th "	E. 45° S.
3d "	N. 8° 53′ E.	9th "	E. 78° S.
4th "	N. 10° 7′ W.	10th "	E. 67° S.
5th "	N. 23′ W.	11th "	S.
6th "	E.		

A Tabular View of Section III. representing a Survey 6 Miles East of Brandon. Breadth of Track, ¾ Mile. Course of Storm, N. 56¼° E.

1st Group.	N. 56¼° E.	8th Group.	N. 78° W.
2d "	N. 29° 24′ E.	9th "	W.
3d "	N. 11° 15′ E.	10th "	W. 45° S.
4th "	N. 6° E.	11th "	S.
5th "	N. 10° W.	12th "	S. 22° E.
6th "	N. 22° W.	13th "	S. 33° E.
7th "	N. 33° 45′ W.	14th "	S. 45° E.

The curtate cycloids are constructed to exhibit approximately the above tabular views. The radius of the generating circle is to that which describes the curtate cycloid as 1 : 2. The blank in Section II. in the front of the loop is filled out in Section III., while the deficiency in the latter can be supplied from the former. The arrows with cross-bars on the left of the loop designate trees, which, though lying side by side with those on the right of the same loop, yet rested with their limbs over the latter. The barred arrow, in Section II., which lies at the intersection of the curve, was resting upon six large trees.

An involute curve tending rapidly towards the centre might answer tolerably well the conditions of Section II., but fails entirely when applied to Section III. The involute action does not appear to have been distinctly marked. The upward velocity at the axis, though sufficient to elevate heavy bodies, was not a noted characteristic of this tornado. The horizontal prostrating force was entirely paramount, and in some places acted with terrific energy. Bodies fell before it as if struck by a thousand battering-rams.

The data thus far given are believed to be sufficient to enable the reader to form his own opinion intelligently of the mode of action of this tornado. A few observations may afford additional aid.

1st. The track of greatest violence, between the dotted line and the right border of the storm, was clearly defined, especially on the left, where the tornado first encountered the forest; but after plunging into it for some distance, the action became more obscure and less symmetrical.

2d. On the left of this line the destructive effects were much less, and generally due to the reverse action.

3d. On the right side of the track, no instance was found of an object thrown inward more than eleven or twelve degrees. The trees almost universally lay parallel, or nearly so, to the course of the storm.

4th. Along the dotted line the trees lay generally N. 23° W., making an angle of 79¼° with the course.

5th. Very few cases were observed of objects in the axis thrown forward in the direction of the path. Their usual position was across the path, and nearly at right angles to it.

6th. No evidence could be found of that upward explosive action sometimes observed.

7th. Observers who saw its approach describe it as a mass of vapor reaching from the earth to the clouds, and whirling in utter confusion. None of those just outside of the track noticed any particular disturbance of the prevalent wind.

8th. A heavy fall of rain followed the tornado, equally abundant at the distance of several miles from the track as in its immediate vicinity. As far, indeed, as can be learned, there was on that day, in those parts of the State visited by this great southwest current, an unusual fall of water.

9th. A sudden change in temperature succeeded the storm, with the wind from northwest. The thermometer fell, in from twenty-four to thirty-six hours, to 8°, a reduction of sixty degrees in a little more than one day.

Rate of Progress.

Very imperfect evidence of this could be obtained along the part examined by the writer. The time of the passage of the tornado over any one place was variously stated at from ½ to 1½ minutes. Mr. Coleman remarked that he saw its approach at a distance of three fourths of a mile, and scarcely took five steps before he was struck down.

Assuming the diameter of the storm at three fourths of a mile, and the time of its passage one minute, would give forty-five miles per hour as its rate of progress.

Seven miles northwest of Little Rock, Arkansas, it rained on the 19th of January, with a warm wind from the southwest. On the morning of the 20th, it was clear and cold; wind northwest. The minimum pressure of the barometric wave passed Oxford, Ohio, at 12½ P. M., on the 20th. If it passed the place named in Arkansas at the same hour on the 19th, then the progressive velocity must have been thirty-three and a third miles per hour.

Mr. Raiff of Sandyville, Tuscarawas Co., northeast by east from Brandon sixty-seven miles, states, in a letter to the writer, that the tornado passed near that place at 3½ P. M. Its terrific roar burst upon Brandon as the clock was striking 2 P. M.

This would give one hour and a half for a progress of sixty-seven miles, equal to forty-four miles and two thirds per hour. This is doubtless a high progressive velocity, but the facts in the case seem to justify its adoption. On the assumption that the storm moved in a circle with a progressive velocity one half of the rotary, then the rate of motion on the right of the path would have been nearly ninety miles per hour. This estimate is probably not far from the truth for the right-hand border; but the velocity near the axis must have been much greater, owing to the involute form of the curve. This velocity may be reached approximately by another process.

A little in advance of the forest where the survey in Section III. was made, there stood, on ground sloping gradually towards the coming storm, an open grove of giant trees, mostly oak. They seemed like an advance-guard to the forest beyond, and from their size and strength to defy any attack. The tornado swept up the slope and struck them with appalling fury. Scarcely one remained standing. They were uprooted, broken, or shivered to fragments. One of them, eighty feet high, three feet in diameter, and sound to the core, was broken off near the ground. The force necessary to produce this effect can be readily determined. It evidently falls under the case of a beam fixed at one end; and, standing perpendicularly, its own weight is not to be taken into account. The following data are sufficient for the solution of the problem. Height, eighty feet; diameter, two and a half; top in outline, fifty feet high by forty in width. Neglecting the lower part of the trunk, the resultant of all the forces acting upon the tree will be at a distance of fifty-five feet from the point of fracture. Assuming the strength usually assigned to oak, it will be found that 73,636 lbs. would be required to break the tree. The surface included in the outline of the top (forty by fifty) would be two thousand feet. If we suppose that the network of limbs, twigs, &c. actually covered one fourth of this, then the area on which the wind acted would be equal to five hundred square feet. Hence the pressure upon each square foot would be 147 lbs. Estimating the force of the wind to vary as the square of the velocity, it would require, to produce the above pressure, a velocity of 171.6 miles per hour. This is 251.6 feet per second, nearly one fourth the velocity of a cannon-ball. As enormous as this velocity may seem, there appears no good reason to believe it far from the truth. The result is probably below rather than above the true value. The only part of the calculation liable to material error is that relating to the surface exposed by the top. Taking into account the state of the trees at this season of the year, destitute of leaves, it is believed that the surface assumed to be exposed to the wind is greater than was really the case. Other circumstances are not wanting to confirm this view. A piece of board was driven three inches into a charred stump, and the writer found a shingle driven three fourths of an inch into an oak-tree. A mass of brick cemented together, containing twelve cubic feet, and weighing more than 1,000 lbs., was carried fifteen feet from the wall of a house.

The aggregate force exerted by this tornado at any point along its path, if clearly stated by comparison with some well-known standard, would doubtless appear startling to any one not conversant with the subject. The following statement will, it is believed, be considered sufficiently cautious by those who will take the trouble to make the calculation. The *force* exerted across a section of this tornado half a mile wide and one hundred feet high *was equal to half the steam-power of the globe.* In the ten miles of the track examined by the writer, there must have been prostrated or broken, in about twenty minutes, not less than 50,000 trees.

One of the most interesting problems connected with this subject, and perhaps the most difficult of solution, is not only the generation of this amazing force, but its renewal for hours against such a host of opposing obstacles. These questions it is not the intention of the writer to discuss at this time. He has no theory to propose or to sustain in reference to the mode of action of tornadoes in general. After various suppositions and trials, and after a careful examination on the ground of the phenomena of this storm, he entertains no doubt that it was an involute whirl of great violence, remarkable for its symmetry, and for the rapid progressive and rotary velocities. The curtate cycloid was adopted, because it seems to be the only curve which embraces all the facts in the case.

The general course of the tornado, and the counties in which its presence has been ascertained, are also laid down on the map. The shaded part of the path represents the known portions; the dotted lines, the supposed route, either above the earth, or, if on it, too diffuse in action to attract special attention.

The tornado left the earth in the eastern part of Knox County, and skipping over Holmes, it again descended to the ground in Tuscarawas, with nearly, if not quite, its original violence, and extended through most, and perhaps all, of Carroll. Its course is somewhat curved, presenting its convex side towards the north.

Its place of beginning was 40° 18′ N. latitude, and 5° 30′ W. longitude from Washington.

I have not yet been able to learn whether the tornado which passed over Washington County, Pa. on the same day was a continuation of this. The probabilities are against this supposition; for the northern line of Washington County, Pa. and Harrison County, Ohio, nearly coincides; and the Ohio storm, to have reached this portion of Pennsylvania, must have curved rapidly towards the south.

4. An Account of a Tornado which passed over the State of Connecticut on the 9th of August, 1851. By Professor John Brocklesby, of Hartford, Ct.

On the 9th of August, 1851, a storm of extraordinary violence swept over the northern part of the State of Connecticut, and a portion of the southern border of Massachusetts. It was, indeed, felt elsewhere, but throughout this region its passage was marked by all that fearful and destructive energy which characterizes the march of the hurricane. A record of its phenomena may therefore be of use as a contribution to our stock of meteorological knowledge.

The southern wing of the hurricane swept over the city of Hartford, causing comparatively little damage, while the northern extended to Springfield and its vicinity, where it was equally harmless. Its greatest power was felt in those towns in the State of Connecticut which lie northerly and easterly from the city of Hartford.

Soon after the storm, I visited a considerable portion of the tract over which it had passed, and, by means of conversation with intelligent observers, private correspondence, and public accounts, obtained at the time much reliable information and many important facts respecting this tornado. The points to which my attention was most particularly directed were principally three; — 1st. the time of the occurrence of the storm; 2d. the direction of the wind; 3d. the evidences of a whirling motion. Unfortunately, no record of the state of the barometer could be obtained at any locality where the storm prevailed.

New Hartford. — Beginning at the westerly side of the storm track, we find that the power of the tempest was first felt at the town of New Hartford, situated about twenty miles west of the city of Hartford. The wind here blew from the southwest, and with such force as to cause a general destruction of property. Buildings were blown from their foundations, roofs torn off, and trees prostrated, their heads lying towards the northeast.

Canton. — At Canton, the next town towards the east, my informant, Mr. Ryder, a resident and an eyewitness, writes as follows: "The place of my abode is a valley locked in by mountains of moderate elevation. The tempest occurred about ten or fifteen minutes before five o'clock in the afternoon, the storm-cloud presenting an unbroken concave front. The rain first came in sight in the northwest, about one mile distant from the observer. A minute afterwards it appeared on a mountain south of west, at nearly the same distance. The rain from these two quarters had almost met in the centre of the valley, when the storm appeared on a mountain to the west, leaving a dry space half a mile in diameter inclosed by rain, but which erelong was swept over by the storm. It was at the moment when the rain was from a quarter to half a mile distant from me that the first current of wind approached, blowing fiercely from a point south of west, and prostrating crops, fences, &c. in its course. This lasted probably about three minutes, when a strong and powerful current, apparently more violent than the first, came from the northwest. A scene most terrific and awfully grand was now presented to view, as the two currents rushed together and passed over the eastern mountain, levelling the forest as the mower's scythe sweeps down the grass of the field. Not one fourth of the trees were left standing, and those only the most flexile, that had bent to the power of the storm. When the opposing winds came in conflict, a very violent whirling was noticed. No hail of any amount fell, nor was the thunder unusually loud." The time that the tempest lasted was variously estimated at from five to ten minutes, but my informant thinks that the actual duration was about seven minutes.

Simsbury. — The storm occurred at Simsbury, a town east of Canton, at about five o'clock, P. M. In the southern part of the town the wind was from the southwest, and in the northern part from the northwest. The violence of the wind was here very great. The entire roofs of several buildings were torn off, and one common-sized barn was moved eighteen or twenty-four inches from its foundation. A fine timber lot was here completely destroyed, trees a foot and a half in diameter being broken short off fifteen or twenty feet from the ground. Hail fell of moderate size.

Avon. — In Avon, a town joining Canton on the east and southeast, the gale was far less severe. Where it swept along with the greatest fury, it appeared to consist of narrow currents a few rods wide. In one cornfield a row or two of corn were carried completely away, and not even a stalk of it found, while the rows on either side were scarcely injured. The wind was from the southwest. The head of the trees which were blown down fell towards the northeast.

Bloomfield. — A mountain range, known by the name of Talcott Mountain, and rising to the height of about four hundred and fifty feet, separates the towns of Simsbury and Avon from those of Hartford and Bloomfield on the east. The mountain sinks very much between Simsbury and Bloomfield, and through this gap the wind appears to have rushed with great fury, falling with irresistible power upon Bloomfield and the town of Windsor, which joins Bloomfield on the east. In Bloomfield the damage was general to buildings, orchards, and crops. Houses were unroofed, the roofs being carried to a considerable distance. A large number of sheds, barns, and out-buildings were either partially or totally destroyed. Trees of the largest size were uprooted, and loaded teams blown from the road.

Windsor. — Upon Windsor the full force of the tornado fell. The wind blew from a point west of north, prostrating trees and crops in a southeasterly direction. It was here that I investigated, personally, the track of the tempest. Its breadth from north to south was nearly two miles, and the entire region was covered with wrecks. Numerous houses, sheds, and barns were unroofed and otherwise injured. Some of the latter were moved entirely from their foundations. Forests, orchards, and crops were levelled to the earth, while hail of large size falling upon the tobacco-fields completed the devastation. Large elms, varying from two feet and a half to three and a half in diameter, were blown down, the mass of roots and earth rising in some instances to the height of ten feet above the surface of the ground. The force of the wind was enormous. A large brick house, belonging to Mr. Halsey, afforded sad evidence of the resistless might of the gale. The wind struck the west end of the house, bursting in nearly all the bricks and mortar above the garret floor. The floor was crushed by their weight, and the rubbish precipitated into the room below. At the same time it unroofed the house, and swept away the east wall of the garret, hurling the bricks upon the roof of the kitchen, and thus breaking it through. The manner in which this house was injured showed that the ruin must have been effected by a ***rectilinear*** wind moving with immense force. In another part of the town the direction of the wind was indicated by the way in which it swept through the house of Mr. Ellsworth Phelps. At the first shock, the west front door was burst open, the wind passing through and tearing out the door on the opposite side, with its casements. A bookcase, with every book in it, was carried into the garden; the chairs, tables, and similar articles were all blown out; in fact, the rooms were completely cleared, the wind leaving only the legs and one half of a table. One instance occurred of loss of life; a woman of the name of Alderman being killed by the fall of a chimney, which was blown down by the gale.

Hartford. — At the city of Hartford, which is situated to the south of Windsor, a dark cloud was seen hanging over Talcott Mountain, eight miles westward, at about 5 P. M. In ten minutes the southern wing of the tempest swept over the city. There was not much thunder and lightning, but the wind rushed along in fierce gusts, while the rain descended in torrents, and showers of hail fell. Scarcely any damage occurred. A few trees were broken, and one roof started, and this was all; for the tornado in its might passed to the north of the city. The wind was from the southwest during the early part of the afternoon, the thermometer standing at 82½° Fahr. at 5 P. M.; but as the storm approached, there was a rapid fall of the mercury, the thermometer sinking to 62½° Fahr. by 5½ o'clock, the wind then blowing from a point north of west. In the western part of the town of Hartford much damage was done, though not so much as in Windsor. Some buildings were blown down, others unroofed, and considerable injury done to the orchards.

Suffield. — In respect to the town of Suffield, which joins Windsor on the north, one correspondent, Mr. Wm. G. Loomis, writes as follows: "At about 4 P. M., dark and heavy clouds were noticed, which gradually rose above the line of the western hills (the Talcott Mountain range), two and a half miles from the town, while loud and repeated thunder was heard. At about 4½ P. M. the storm-cloud appeared, of a *light-green color*, and soon the rain was seen pouring down in sheets, while the storm advanced swiftly eastward. At ten minutes to five it fell upon the village, increasing in violence until five o'clock. At about 5¼ P. M. the storm was over. The wind was due west during the first and most violent part of the hurricane, and continued in this direction for the space of ten minutes. During this period showers of hailstones fell, and all the damage that occurred took place then; after a moment's cessation, the wind blew from the north, and next from the northeast, decreasing in violence with each change. The crops of tobacco and corn which were prostrated by the hurricane lay from west to east, and the uprooted trees fell likewise almost in a line with the compass from west to east, and in no other direction." Another observer, Mr. S. A. Cooley, thus describes the phenomena of the storm in a different part of this town: "The wind was from the south until about 5 P. M., when it changed suddenly to the northwest, and a small black cloud appeared in the northwest, spreading to the north and south, and growing extremely black, while beneath this dark cloud there was one of a *peculiar green color*." According to Mr. Cooley, the storm broke over his station at about 5 P. M., as soon as the ends of the dark clouds, extending around from the north and south, had met in the southeast. Upon the sudden change of the wind to the northwest, the rain immediately commenced, and soon after hailstones fell of the size of nutmegs. The trees were, for the most part, prostrated towards the east and southeast, but more generally to the south of east. The northern part of this town suffered severely, while in the southern the gale was said to be comparatively harmless. The hail was very destructive to the tobacco crop. The loss arising from this cause was estimated at $10,000.

Enfield. — Crossing the Connecticut eastward, we find the tornado falling with different degrees of severity upon a region of considerable extent, embracing the towns of Enfield, Somers, East Windsor, East Hartford, Ellington, Vernon, Manchester, Tolland, Bolton, Coventry, Mansfield, Chaplin, Hampden, Windham, and Andover. Upon the town of Enfield, east of Suffield, the storm swept down with violence. According to information received from this place, the afternoon of the 9th of August was very hot, and this state of the weather continued until 4 P. M., when the wind suddenly veered round to the northeast, and blew quite fresh and cool, so that the mercury fell 10° Fahr. in less than five minutes, and in the course of forty minutes had sunk 20° Fahr. The storm burst upon this town a few minutes before 5 P. M., the general direction of the wind then being from the northwest. The crops of corn and tobacco swept down by the storm in the vicinity of Mr. Johnson's (my informant) residence indicated that the direction of the wind varied from west-northwest to north. Local whirls were observed, but there were no indications of any general rotatory motion of the storm. The hail passed in veins of greater or less width, a very great difference in its effects being found in a distance of a dozen rods, or even less.

Somers. — Over Somers, a town east of Enfield, the tempest swept with desolating power, prostrating trees and field crops, the latter being also severely injured by the hail. With such force did the hailstones descend, that in one locality they pierced through the window curtains as well as the glass, indenting even the woodwork of the buildings. The tobacco crops within the range of the storm were entirely destroyed, and other crops seriously injured; the corn was literally cut down.

The Line of the Willimantic Railroad. — The Willimantic Railroad, which runs from Hartford to Willimantic in a direction a little south of east, passes through a portion of the State where the tornado raged with full fury.

The railroad at Andover was so blocked up with fallen branches and fences that the evening train was stopped, and an additional locomotive procured to bring it on. In this town, along the line of the railroad, the wind made a clean sweep of trees and fences for the width of twelve rods, through a distance of two or three miles, in a direction parallel to the railroad track.

At Bolton, near the Notch, which is a gap in a mountain ridge, where the railroad passes through by a deep cut, a tall maple grove was stripped of its branches, the trunks of the trees being left, some forty feet in height, evenly shaved of their limbs.

Coventry, Chaplin, and Mansfield. — In Coventry, Chaplin, and Mansfield, towns lying east of Bolton, the tempest was accompanied with an unusual quantity of hail. In Coventry the limbs of trees, twenty feet long and six inches in diameter, were torn from the trunks and carried to a distance of several rods, and trees were torn up by the roots. According to the observation of Mr. Boyton, the course of the wind in the southern part of this town was from west-northwest. Trees were prostrated, some towards the east and southeast, the greater part falling south of east. Large trees were here twisted off without tearing up the roots, showing the existence of a whirl in this locality. The storm commenced at 5¼ P. M. In the northern part of Coventry hail fell in unusual quantities, destroying much glass, and the tempest was more destructive than at the southern borders. An eyewitness of the desolation caused by the tornado on this part of its track thus writes: "The hail on the hills of North Coventry was very severe. Scarcely a pane of glass on the north and northwest sides of the houses escaped being broken. I cut a corn-stalk which was a fair sample of the state of the corn crop; it was battered and splintered almost to shreds." A gentleman states, that at Mansfield, a town east of Coventry, on the morning after the storm hailstones were picked up which measured three inches in circumference, and at Chaplin on the same day, at noon, a number were found frozen together in a mass eight inches thick. During the storm at the latter place, hailstones were picked up as large as hens' eggs. More than four thousand panes of glass were here broken by the hail, and in the neighboring town of Vernon the loss in glass from this cause was estimated at from $ 3,000 to $ 4,000. There appeared to be here a narrow vein of wind sweeping from the northwest. At Manchester there was less hail and more wind than in the adjacent towns.

Windham. — At Windham an immense elm, four feet in diameter, was so bent and twisted by the force of the tempest, that a large splinter three feet in length was split out of the centre, when, the parts of the tree springing back again, it appeared as sound as ever.

Hampton. — The blowing down of a barn at Hampton showed that the gale reached this town.

Springfield, Mass. — In Springfield, Mass., the storm was heralded by the approach of a dark, threatening cloud in the west, at about 5 P. M. The tempest did not begin with a burst of violence, but gradually increased in fury until the water came down in torrents, while the wind blew almost a hurricane, and the lightning flashed incessantly. Hailstones fell of the size of bullets, but not in large quantities. In the vicinity of this city, and in Holyoke, the lightning was frequent and destructive, striking and setting fire to several houses and barns, and killing two or three persons.

The rain reached Northampton, but does not appear to have extended much to the north of this town. It was stated in a newspaper paragraph, that the storm was felt as far south as Washington, but I have not met with any circumstantial accounts.

The westerly limit of the tornado was not far from the Connecticut River. At Pittsfield a little rain fell, but at Albany it was heavy. Easterly we hear of the tempest at New Bedford, Mass., where it occurred about seven o'clock. A strong wind here blew from the northwest, accompanied by a deluge of rain, while the thunder at intervals was long and loud. No damage was done to buildings or trees, except the removing from the latter a portion of their fruit. But upon the town of Westport, situated twelve miles southwest of New Bedford, the storm fell with the same fury as upon Windsor, levelling trees and crops to the earth, sweeping away fences, and in some instances even overthrowing stone-walls. The blast was attended with a violent storm of hail, which completely riddled and cut up the leaves of the vegetables. Much glass was broken by the hail, and even window-frames considerably injured. Some of the hailstones measured about three quarters of an inch in diameter. The tornado swept over a track about three quarters of a mile wide, rounding in a southeasterly direction.

At Providence we find that at 7 P. M., on the 9th, a fierce squall of wind fell upon Providence River, which capsized three boats, causing the death of four ladies.

Inferences.

The facts obtained in respect to the storm of August 9th, 1851, appear to warrant the following conclusions : —

1st. That, at the westerly border of the desolated track, two strong and opposing winds met each other, for in New Hartford, Canton, Avon, and the southern part of Simsbury, trees and crops were levelled by a southwest wind ; while in the northern part of Simsbury, and in Canton, a violent northwest wind simultaneously occurred. In the latter town the *two winds were seen to meet.* Canton and Simsbury were the arena of the contest, when, the northwest wind prevailing, it swept on triumphantly through the gap of the mountain at Simsbury, passing over, in its devastating course, Bloomfield, Windsor, and the other towns already mentioned lying east of the river. The sudden fall of the thermometer shows that the northwest wind obtained the mastery, descending to the earth as a surface wind. It arrested the progress of the southwest wind as a lower current, for on the north side of the tempest-track there is no evidence of the presence of a southwest wind near the earth.

2d. The direction taken by the falling trees and crops, and the manner in which the destruction of buildings was effected, give no evidence that the general devastation was caused by the progressive advance of a cyclone or whirlwind of small extent, like that which passed over New Haven in the year 1839. It is true, that evidences of the existence of whirls were noticed in one or two localities, but these had undoubtedly a local origin. The general phenomena were entirely different. Crops and trees were blown down in one direction by the sweep of the prevailing wind. The houses were not destroyed by the walls and roofs bursting outwards from the expansion of the air within. In the case of Mr. Halsey's house the wind evidently forced in the west brick wall of the garret, and, being momentarily detained by the opposite wall, gathered its strength, threw off the roof on either side, and burst through the eastern end. The clearance made by the wind at Mr. Phelps's house was caused in the same way. Throughout the whole track of the tempest its ravages appear to have been effected by a solid column of wind advancing from the northwest to the southeast, and sweeping down in its direct and impetuous charge all opposing obstacles in the line of its path.

Force of the Wind.

That this was very great is proved by several circumstances, some of which have already been detailed. At Simsbury, trees a foot and a half in diameter were broken off like pipe-stems, at a height of from fifteen to twenty feet above the surface of the ground. The brick wall of Mr. Halsey's house above the garret was blown in. A leverage was probably here obtained, as these walls are not always fastened at the top by being connected with the rafters.

In Windsor, we have also an instance of an empty ox-cart being overturned by the power of the wind; and when we consider the broad base of the vehicle, its great weight, and the comparative small surface it presents, we see that the force of the blast must have been very great. In the same town a physician, who was riding in a buggy with his wife, and driving before the gale, felt distinctly that the carriage was several times tilted up from the ground by the power of the blast.

The only fact which enables us to make an approximate estimate of the force and velocity of the wind, is the following.

In West Hartford, where the storm did not rage with its greatest fury, a man was blown off from the ridge-pole of a barn, and carried to the distance of eight or ten rods, without injury. The entire outer surface of a person is estimated on an average at about thirteen or fourteen square feet. If the man had stood erect upon the barn, half of this surface would have been presented towards the wind; but under the circumstances, he would take such a position as to present as small a surface as possible to the direct force of the gale. Let us suppose this to be five square feet, which is probably a high estimate. Now, taking the weight of the man at one hundred and fifty pounds, we shall find that simply to have moved him in a horizontal direction a force a little greater than thirty pounds on the square foot must have been exerted. A wind having a force of thirty pounds on the square foot possesses a velocity of about eighty miles per hour.

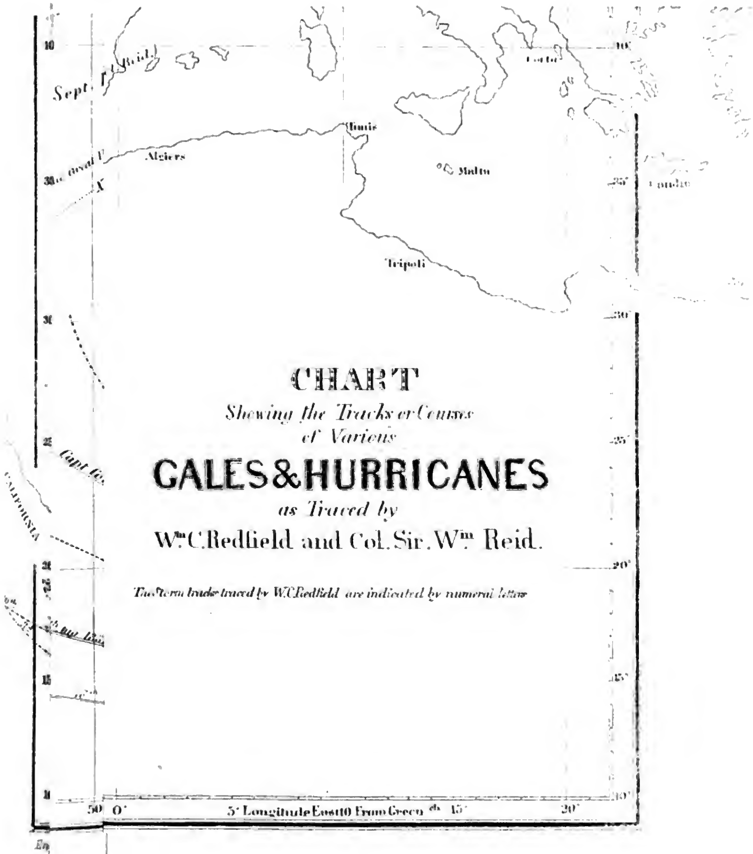
The Hail.

The facts show that the hail fell in the greatest quantity, and of the largest size, on the northern border of the storm-track. In the centre of the track hail indeed occurred, but not to any great extent, while on the southern border but very little fell. I am inclined to account for these circumstances in the following way.

The sudden irruption and descent of the cold northwesterly blast effectually arrested the progress of the warm southwesterly wind as a lower current, presenting to it at the surface of the earth a perfect barrier. A vast volume of warm air was thus driven up into the atmosphere on the southern border of the northwest current, the south wind overriding the latter, and bearing the hail, formed in the higher regions of the atmosphere, over to the northern border of the storm-track. The energy of the opposing winds, their sudden conflict and varying temperatures, fully account for the large quantity of hail that fell, as well as for its unusual size.

5. CAPE VERDE AND HATTERAS HURRICANE, AND OTHER STORMS, WITH A HURRICANE CHART. By W. C. REDFIELD, of New York.

THE discovery of the rotation and determinate progression of storms, which was made by the author in the year 1821, has induced the occasional publication of some of the specific results of his inquiries, and has led others to engage with success in this important and inviting field of investigation. On the present occasion it is proposed to



CHART

Showing the Tracks or Courses
of Various

GALES & HURRICANES

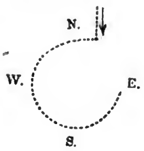
as Traced by

W.ᵐ C. Redfield and Col. Sir. W.ᵐ Reid.

The Storm tracks traced by W.C.Redfield are indicated by numeral letters

give some account of the earliest autumnal hurricane of 1853, in the North Atlantic ; for, in consequence of the otherwise equable character of the weather which then prevailed, this storm was readily traced from near the western headlands of intertropical Africa to the southeastern shores of the North American States; and thence on a northeastwardly course to the polar basin, while touching both the American and European shores of the Atlantic. During this vast progression, the storm is found to have presented the cyclonic characters that are common to other gales; and the observed extent of its route may aid in correcting the somewhat prevailing impression, that these storms have their chief origin at, or near, the islands of the West Indies, or at other quite limited localities.

The evidence of the normal progression and rotation of this storm, as derived from marine observations, is comprised in a series of more than one hundred reports. The earliest of these reports is of August 30 – 31, in Lat. 13° N., Long. 29° W. The second report, September 1, Lat. 15° 40′, Long. 48° 20′, shows a violent hurricane, beginning at northeast, which marks the centre-path of the storm ; its course being northwestwardly in these latitudes. Its ending here with wind at east-southeast was evidently due to the northwardly progress of the ship. Report 5, as subsequently corrected, shows a fall of the barometer to 27.30 inches, on September 3, in Lat. 18° N., Long. 53° W. In Lat. 27°, Long. 69°, as seen in Report 8, the gale commenced at east-northeast, and had the course of the vessel been northward, the gale must have veered by the east, in its progressive changes, as was seen in Report 2. But in running *south*, the gale had become a full hurricane at 10 P. M., and the vessel was then scudded before the wind, and thus crossed the centre-path of the cyclone in running round its axis. Had the progression of the hurricane been sufficiently slow, the vessel might thus have completed an entire circuit. Such examples may be useful to those who hesitate to admit the rotation of gales.



The rate of progression, as in other storms, appears to have lessened as the hurricane approached the axial belt or parallel where the diurnal movement in the inferior portion of the lower atmosphere becomes relatively equal with that of the earth's crust; which, on this occasion, appears to have been somewhat above the thirtieth degree of latitude.

The progression of the gale, as in former cases, appears to have been greatly accelerated after it passed the axis of equal diurnal motion, on its recurvated course through the temperate and higher latitudes. The following estimates are roughly made, by setting off the progression on the Chart, as shown in Trace XXIV., reckoning in English miles, at seventy for a degree of the meridian.

	Hours.	Miles.	Av. per hour.
From the position of the William Money (1) to that of the Hermann (3), about	84	1,942	22
To position of Ocean Bird (8)	50	980	19.6
“ Georgia (20)	62	814	13
“ Addy Swift (27)	7	175	25
“ Independent (49)	36	1,102	30.6
“ Avalanche (90)	30	1,505	50
“ Virginia (104)	15 (?)	758	50.6 (?)
	284	7,276	

We have thus an estimated distance of 7,276 miles, traversed by the storm in about twelve days, at an average rate of progression of nearly twenty-six miles an hour. The slower rate of progression at and near the axis or belt of equal diurnal motion, where the storm recurvates, accords with results ascertained in my previous inquiries, and with those severally shown by Reid, Thom, and Piddington, in the gales of the Southern Ocean and the Asiatic Seas. In the approximated delineation of the axis line or centre-path on the Chart, reference was had to the path of greatest violence, and especially to the opposite veering of the wind, which is found on the opposite sides of an advancing cyclone.

Eastward of the Grand Bank, nearly all the reports were from the right side of the storm-path, and, so far as appears, mostly at great distance from the true axis path of the gale. That the cyclonic nucleus of the storm had become greatly enlarged, and that it pursued the general course I have indicated, appears from its wide-spread violence, and the intensity and extent of its influence on the barometer, and is also shown by the most northern reports. From these reports we see its violence extended beyond Lat. 60°, in the general direction of the Feroe Islands and the main entrance to the Arctic Sea.

In other cases we find, that, after passing the latitude of Bermuda, the expansion of the storms is so great that their southern portions often advance nearly from west to east; but reach the successive meridians no sooner, and sometimes even later, than the axial portion of the gale, which pursues a more northeasterly course. And thus in the present case, east of Long. 60°, and between Lat. 35° 30′ and Lat. 42° 30′, a belt of seven degrees, we have a series of thirteen observations, carrying us east to Long. 15° 25′, in Lat. 36° 50′; almost to the southwestern extremity of Europe. If, instead of the broad range of our present inquiry, we were limited, as in the United States, to fewer parallels of the temperate latitudes, how readily might a mere east progression of the storm be shown by these partial observations, and its true course remain unnoticed!

This southward expansion of the storm may have been due to something more than the centrifugal force of the cyclone, acting against the statical pressure of the circumjacent atmosphere. In such a widespread cyclone, whose diameter on the 9th of September extended from Newfoundland to beyond the Azores, or more than 1,500 miles, how could its vast entireness be much longer maintained, against both the centrifugal and gravitating forces of the earth, acting in opposite directions, and with opposite degrees of effect, or predominance, on the sides respectively nearest the equator and the pole? May we not suppose that the southern portion of the gale was in process of separation or falling off toward the equator, and thus supplying the influx which sustains the inferior stratum of trade-wind in Northwestern Africa and the Eastern Atlantic? And is not such a view supported, in some degree, by the gradually altered course of the nucleus of the storm, which, in becoming more free from the equatorial influence, is left to pursue its course toward the polar basin under the predominating influence of direct gravitation?

The unfailing mechanical effect of the cyclonic wind in producing a fall of the barometer within the area of its circuit, and greatest in the axial region of the cyclone, is clearly seen in this storm. The following are the cases reported in which the barometer fell below twenty-nine inches, to which is annexed an estimate of the probable distance of the vessel from the axis of the storm at the time of nearest approach.

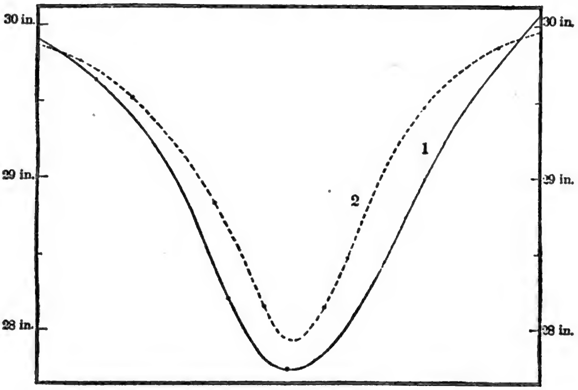
	Minimum of Barometer. Inches.	Supposed Distance from Gale's Axis.
Hermann (5)	27.30	*near.*
Georgia (20)	28.20	45 miles.
Swan (25)	27.94	30 "
Independent (49)	27.75	220 "
Robert Kelly (68)	28.15	265 "
Southerner (74)	28.27	240 "
Larpool (85)	28.52	315 "
Avalanche (90)	28.50	430 "
Andes (96 *a*)	28.48	280 "

From observations of the barometer and winds taken at various points in the United States near the Atlantic coast, and from those made at the signal station in Bermuda, on the right margin of the storm, it appears that the gale was but little felt at the latter place, except as exhibiting the true cyclonic wind, from the 6th to the 8th, from southeast, veering gradually from southeast to southwest, as the bearing and progression of the storm became changed, with a force of wind marked from 2 to 4; the barometer at 30.10, at noon of the 5th, 6th, and 7th, and 30.07 at noon of the 8th. The left side of the storm encroached to some extent upon the eastern borders of North Carolina and Virginia. At Scuppernong, N. C., Lat. 35° 50′, Long. 76° 20′, the cyclonic wind blew from northeast, with a force marked 3 and 2, with rain; and the lower stratum of clouds (the true storm-scud or cyclonic stratum) flew rapidly from northeast; *the upper clouds quite still.* At 9 P. M., the wind had veered to southwest. No observations of barometer. At Fort Monroe, Va., the reported direction of the storm-wind and cloud is the same as at Scuppernong, with rain from southwest at 9 P. M., with thunder and lightning.

At Savannah, on the 5th and 6th, then in front of the storm, the maximum of the barometer was 30.20 and 30.19; on the 7th, with the storm to the east, the minimum of the report is 30.06; and on the 8th, under the rear portion of its annular wave, 30.21. At Jacksonville, East Florida, nearly the same; as also at Charleston, S. C., nearer to the path of the storm; and scarcely falling below thirty inches at New York and Nantucket, as the storm passed.

The annexed diagram represents, in its horizontal scale, the distance of 840 miles between Washington and Bermuda. The full line (1) repesents, approximately, the barometric curve through the centre of the storm, ***transversely to its path.*** The comparison of this transverse curve with the usual curves of progression is of some interest. This diagram shows also, in broken line, the transverse barometric curve through the centre of the Cuba hurricane of October, 1844, when in nearly the same geographical position. This curve is approximated from twenty-eight observations in the path of that storm.

Barometric Storm-Curves, transverse to the Progression.



1. Transverse centre-curve of Cape Verde and Hatteras hurricane, Sept. 7, 1853.
2. Transverse centre-curve of Cuba hurricane of 1844, Oct. 6.

The true character of this gale as a cyclone is made evident by the entire series of observations. This is more directly shown by the various observations made on all sides of the storm during its passage between Bermuda and the nearer portions of the United States. The absolute whirlwind movement of the storm stratum, and the increasing rapidity of its sinistroidal rotation ↺ in approaching the axial area, from whatever side of the storm, as well as the direct effects of this increased rotation on the fall of the barometer, are made manifest by direct observation. Nor can these results be evaded by denominating any one portion of the cyclonic wind, on either side of the cyclone, as another current, or distinct gale. The local variations and inequalities of the cyclonic action, and the errors, imperfections, or defects which may exist in the reports, are alike overborne by the amount of evidence, which serves to show the extent and general entireness of the vortical rotation in the gale.

It would be an error to suppose that the gales and hurricanes which have been traced on our storm charts were but exceptional cases of cyclonic action and progression in the winds of our globe. For there is a constant succession of rotary movements, greatly variant in their activity and their visible effects, to which we shall further allude. It is the more violent cyclones, however, that afford us complete evidence of their geographic routes and their continued movement of rotation.

Of this active class, designated as hurricanes, gales, and storms, it is believed that the tracks or routes of several hundred might be added to our storm maps, by carefully collating the records which already exist. It is certain that a large number might be traced from the records and notices now in possession, or otherwise at hand; of which the case presented is but a single example. The storms noticed in the succeeding paragraphs are selected with reference to their peculiar localities, as showing the uniform extension of the cyclonic system in equal latitudes around the globe, rather than for the amount of information possessed regarding their extent and progression.

Gales of the Eastern Atlantic between the Tropics.

A violent hurricane swept over St. Nicholas, one of the Cape Verde Islands, Lat. 16° 33′ N., Long. 24° 20′ W., on the 2d day of September, 1850. Its duration exceeded twenty-four hours; although the chief damage was done in three or four hours, during the morning of that day. All the crops, and nearly six hundred houses, were completely destroyed. The marine accounts from the vicinity show the further progression of this hurricane on the Atlantic, at the rate of eight or nine miles per hour, in the direction which is common to the gales in this parallel of latitude. Other severe gales are known to have occurred in the Eastern Atlantic between the African coast and the West India Islands, of which seven cases were specially adduced, but are passed over in this sketch. Captain Fitzroy, R. N., having experienced the force of these gales in the vicinity of the Cape Verde Islands, confidently warns those who are inclined to be incredulous about a gale of wind being found in fifteen degrees of north latitude, beyond the supposed limits of the hurricane regions.

The foregoing cases, together with those seen on the hurricane chart, are submitted as indicating the general course of progression of intertropical cyclones in the Atlantic, and their frequent identity, as well as systematic conformity, with the cyclones which visit the more western or northern portions of the oceanic basin, including those of the Mexican and Anglo-American States.

Gales of the Eastern Pacific, near the Mexican Coast.

Our direct knowledge of the paths of these gales is necessarily limited; but the interests of an increasing commerce, as well as of meteorological science, claim the notices which follow.

1. The Joseph Butler, on or about the 24th of June, 1850, encountered a severe gale of wind, near Lat. 16° N., Long. 107° W. (260 miles from the shore of Mexico), which carried away her mainmast. We possess no further accounts of this gale.

2. The barque Como, on the 5th of August, 1850, in Lat. 14° 20′ N., Long. 117° W., encountered a severe gale, commencing at north and veering to west and south. Lost sails and bulwarks, and sustained much other damage. These winds denote a course of progression corresponding to that of the hurricanes in the West Indies, and that the vessel was in the left side of the storm-path.

3. NIAGARA'S HURRICANE. — The Niagara was dismasted in a hurricane, September 9th, 1850, about ninety miles south of Acapulco, (Lat. 15° 16′ N., Long. 99° 50′ W.)

The Diana, September 11th, Lat. 22° N., Long. 116° W., took a severe hurricane from northeast, veering to southwest; blew five hours; vessel hove on beam-ends.

The Diana's position was in the left side of the storm-path, but near to the axis line; the progression of the storm being still northwesterly. Its course of progression from the Niagara was thirty-four degrees north of west, or west-northwest nearly. Its rate of progress was nearly twenty-three miles an hour, allowing no error for the nautical dates. Part of the track falls on our Chart. See Track XXVIII.

4. The Laura, September 26th, 1850, Lat. 26° N., Long. 123° W., in a severe gale was thrown on her beam-ends; lost cargo, &c. We have no further account of this gale.

5. The Kingston, from San Francisco for Panama, experienced a severe gale on the Mexican coast, and was thrown on beam-ends,

October 1, 1850, in Lat. 14° N.; and reports that the gale swept the whole coast with great violence; as may be seen in these statements.

The Belgrade, from San Francisco for Realejo. October 1, fine breeze from west-northwest, and heavy swell from southeast. At 10 P. M. wind hauled suddenly to southeast, with increased force and squally appearances; at midnight under single reefed topsails; 1 A. M. still increasing, with vivid lightning and heavy rain; 4 A. M. split fore-topsail; 8 A. M. lost foresail; gale increasing to a hurricane; thrown on beam-ends, with loss of main and mizen-topmasts, with head of mainmast, when the ship righted a little. At 1 P. M., October 2, hurricane still increasing, ship on her beam-ends; lost fore-topmast, with much other damage; at midnight, blowing as hard as ever; at 4 A. M., October 3, more moderate, heavy rain; October 4, Lat. 18° 11′ N., Long. 104° 5′ W., made for Acapulco. The statement shows that this vessel was on the right of the axis path of the storm.

The Galindo, on the same route, experienced a severe hurricane on the 1st and 2d of October; was thrown on beam-ends and dismasted; and arrived at Acapulco at the same time with the Belgrade.

The Lovina, off Cape San Lucas, the southern point of California, October 5, was thrown on beam-ends in a violent hurricane, and lay twenty-one hours.

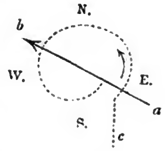
The Fanny, from Mazatlan, in the Gulf of California, for San Francisco, was damaged in the gale, on the 5th and 6th of October, and put back to Mazatlan.

The progress of this hurricane, during four days, appears to have been northwest by west, nearly, at a rate not exceeding eight or ten miles an hour. Part of this track falls on our Chart. Track XXIX.

6. Amazon's Hurricane. — The brig Amazon, from New York for San Francisco, encountered a severe hurricane, October 3, 1850, in Lat. 13° 30′ N., Long. 116° 50′ W., which commenced at southwest, veering successively to southeast, east, north, west, ending at southwest; in which lost main-topsail and foresail. Captain Watt states that the gale was as severe as those in the West Indies. This off-shore hurricane was contemporaneous with that last noticed. A passenger writes that Captain Watt put his vessel before the blast, or "scudded" her, the tempest raging during the night, with momen-

tarily increased fury. It veered from southwest to due south, thence to southeast, thence to northeast and north, and thence to southwest, thus making the circuit of the compass! According to the reckoning, it veered thirty-four points in the space of six hours; during which time the brig was kept before it. She was kept scudding till the hurricane ceased, and was then laid to in a heavy gale from the southwest, which followed the hurricane.

From the above we may infer that the course of the vessel while scudding was not unlike that shown in the annexed figure.



a b Course of the storm's progression.
c Course of the Amazon in the gale.

The short time in which the brig ran entirely round the axis of the gale, after entering its violent portion, shows that its diameter was small, and that its progression was remarkably slow. This slowness is also shown by the manner in which the brig, steering north for San Francisco, was able to *overtake* the cyclone, and run into it, upon its southeastern side, where its wind was southwesterly. Hence, too, after clearing the vortex of the cyclone, and heaving to, the duration of the exterior portion of the gale was so much prolonged, notwithstanding the drift of the vessel was in a direction opposite to the progression of the storm. It is probable that this progression did not exceed four miles an hour, and it may have been less.

This is a slower rate of advance than has yet been found on the Atlantic, but it accords well with other cases which have occurred within the tropics, in the Indian Ocean. It appears, also, as having some relation to the slow rate of advance already noticed in the contemporaneous in-shore hurricane of the Kingston. Hence we may infer, that the great current of progression in which the cyclones are imbedded was at this period and in this region, at least, comparatively sluggish and inactive.

7. Captain Budd, in a steamer from San Francisco, encountered a gale from southeast, October 22, 1851, about sixty miles west of Cape San Lucas, the axis of which recurved northward, bringing the wind to northeast, before reaching the position of his ship. See Chart.

8. Panama's Gale of July, 1852. — The Panama experienced a hurricane, July 16, 1852, in Lat. 15° N., Long. 115° W., which lasted ten hours; carried away top-gallant-masts, yards, sails, &c.

Extract from log-book of ship Empire, bound for San Francisco: —July 19, 1852, commences with heavy gales and bad sea, from the *north;* under double-reefed topsails and courses. [Ship in front part of the gale, to the left of its axis path.] At 8 P. M. heavy gale from north-northwest; at 10 P. M. very heavy gale; hove the ship to under triple-reefed main-topsail; midnight, gale increased to a hurricane; the mainmast went by the board, together with the mizen-mast, fore-top-gallant-mast, &c., with everything attached; blowing a complete typhoon. At 7 A. M. gale had in some measure abated, and at 8 A. M. got the ship before the wind, then blowing from south to southwest. Lat., by account, 17° 4′ N., Long. 117° 35′ W.

This could have been none other than the Panama's gale, moving on a course between thirty and forty degrees north of west; and, if there be no error in the Panama's date, at the rate only of about three miles and a half per hour!

This slowness of progression, in the three hurricanes of the Panama, Amazon, and Kingston, is of great interest for navigators in the Pacific. For it shows how perfectly the exposure and safety of their vessels, during such hurricanes, are placed in their own control; at least, in cases where sea-room on all sides is afforded them. Thus, if the master of the Amazon had comprehended the character of his hurricane, or its law of rotation and progression, he might have run more eastward until the state of the barometer and winds would have allowed him to come up to his desired course. This would have enabled him to make a safe, rapid, and successful run towards his port of destination, while he kept on the outskirts of the gale.

The Empire, when headed off by the north wind in the *front* of the gale, could not pursue her course for San Francisco, nor safely heave to, on either tack. But she had opportunity to run southward in the beginning of the gale, keeping the wind on the starboard quarter, until the state of the barometer and the diminished strength and westerly changes of the wind should enable her to turn eastward, around the rear of the hurricane, and thus regain her course with a fair wind.

The intertropical gales of the North Pacific which are comprised in these few notices, are seen to have occurred in the several months from June to October, both inclusive; and we have account of another violent gale, far to the westward, in the month of May. The prevalence of storms on the Pacific coast in the other months of the year, from October to April, has been noticed by Humboldt, and other writers, and is now but too well known by the experience of navigators.

We thus establish the prevalence of active cyclones upon the southwestern coast of North America at all seasons of the year, and find that these are sometimes of great violence. That many of these cyclones pass over the Mexican territories, some to the Gulf of Mexico, under the local name of ***northers***, and others to the territory of the United States, we can find no reason to doubt.

The very prominent characteristic of southeast winds, in the storms which commonly visit the Pacific coast, affords evidence of their progress *along the coast* in the lower latitudes, and of their direct entrance upon those shores in higher latitudes, subsequent to their northwardly recurvation. These characteristics early attracted the writer's attention, in the gales which are noticed in the voyages of Cook, Vancouver, and others, and in the journals of whalers which came under his inspection.

We might infer, therefore, without reference to other and direct evidence, that the same general system of cyclonic movement prevails on the Pacific Ocean and the continent of North America that is found on the Atlantic. Indeed, a glance at our Storm Chart might afford conviction of this fact.

A competent knowledge of the cyclones, and of the law which governs their development, has become essential to our navigators. Both merchants and insurers are beginning to discover that even the good qualities of a vessel have commonly less influence upon the safety of her voyage, than has the intelligence and skill of the commander. Hence, there are now insurers who freely select those risks which are in charge of the most competent masters; leaving other risks, of whatever class, to underwriters who are willing to rely on the classification of the vessels.

American Storms of December, 1836.

From the 30th of November to the 21st of December, 1836, six great cyclones passed successively over the United States, having passed New York on November 30th, December 5th, 10th, 14th, 17th, and 21st, respectively; under which, the barometer fell .62, .35, .44, .86,

.90, and 1.05 in., in the several cases.* The surrounding waves of exterior pressure raised by their rotation, and separating each cyclone from the other, were indicated by the barometer as follows, viz.: — November 28th, 30.27; December 4th, 30.29; 8th, 30.35; 12th, 30.28; 16th, 30.45; 19th, 30.80; and December 22d, 30.72 inches. Each cyclone presented here the winds of its two right quadrants, gradually veering from a southern quarter to the western board, as it went onward; thus showing the cyclonic centres to have passed far westward of New York, and over the Canadas, in their several routes to the northern regions of the Atlantic.

In the last of these storms, which has been examined by Professor Loomis,† the wind at New Orleans, on the 20th, blew hard from a southern quarter, and also on our Atlantic coast, during the latter part of the 20th and the early part of the 21st, veering westward. At Rochester, N. Y., it blew from southeast on the afternoon of the 20th, with great power, and furiously at Buffalo, also veering round by the south to the west, during the night; thus showing that the axis of this gale passed northwardly at a distance much to the west of these places. This fact is confirmed, also, by the reports of winds as made to the Regents at Albany, and by those obtained from the military posts and other sources; very many of which are given by Professor Loomis. The same fact is shown by barometric observations as published by him. For although the central nucleus of the storm, or area of greatest barometric depression, passed the Western observers during the night, when the greatest and most rapid fall and rise of the barometer was not noted, yet the depression as recorded proves to be greatest as we go towards the true centre-path of the storm, as the same is approximately indicated on the Chart, marked XXVII. This is seen in the observations made at Lexington, Ky., Springfield, O., Marietta, Twinsburg, Rochester, Syracuse, Albany, Montreal, Hanover, and Quebec, which, even as given, show a mean fall of 1.075 in.; while those of twelve places on or near the Atlantic border, from Savannah to Newfoundland, show a mean fall of but .91 in. If the true course of the storm had been from west to east, the fall in the barometer would have been much the greatest on the Atlantic border, owing to the lower level, which is not considered and allowed for in the above estimate, and to a less obstructed rotation of the storm, on reaching the Atlantic.*

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* This series may serve to illustrate the continued succession of cyclones in the United States.

† Transactions of the American Philosophical Society, Vol. VII., New Series, pp. 125 - 164.

Moreover, the barometric minimum was observed at Quebec about as early, on the 21st, as at New York and its vicinity, although four hundred and twenty miles farther to the north, and nearly on the same meridian. This more rapid advance of the central portion of the storm, which has been seen in other cases, proves that the true course of progression was on the general route which I have indicated. The rate of the storm's progression, from noon of the 20th to noon of the 21st, I estimate at about thirty-three miles an hour.

It is true, however, that Professor Loomis has traced this storm *eastward*, from the Mississippi to the Atlantic; and has stated, also, that in this case "there was no whirlwind." But I may state that he had almost no observations other than from the right side of the storm's centre-path. Now, in like limited manner, and *with like directions and changes of the wind*, the great hurricane of September, 1853, which we have just considered, may be traced eastward from its centre-path, for a greater distance, and in the same latitudes, as already shown. The like is also true of the great Cuba hurricane of 1844, as also of Colonel Reid's hurricane, which crossed Bermuda and Newfoundland in September, 1839, (see Chart,) on perhaps the most northerly course that has been traced in any storm.

Yet, who that duly examines these cases will doubt that these storms, in their essential character, were great whirlwinds, moving northward and eastward? Indeed, the same or like phenomena may be traced in every great cyclone that passes over these latitudes.

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* The extreme range of the barometer in a period of seven years at Hudson, O., near to Twinsburg, and about 1,100 feet above tide, as given by Professor Loomis, is 1.719 in., while the range observed in New York during the same period was 2.25 in. Difference, .531 in. The mean of the annual ranges at Hudson during the same period appear to have been 1.402 in., while the mean of the annual ranges at New York was 1.874 in. Difference, .472 in. It appears, therefore, that near half an inch should be added to the depression of the barometer in some of the Western observations of this storm, in order to a fair comparison of the barometric indications with those on the Atlantic border in the same storm.

This eastward extension appears due, in part, to the enlargement of the cyclone; and while affecting its external form, and that of the lines of equal pressure, it does not essentially change the rotative movement; as may be seen by the continued development of the cyclonic winds, and their influence on the barometer.

It is well known that other and similar tracings from west to east have been made of the progress of various storms in the United States. It is believed, however, that the clew to these cases is already afforded, and that many or most of these storms were true cyclones, with orbital courses really analogous to those which are seen on the Chart.

WHAT ARE CYCLONES? — The term Cyclone was first proposed by Mr. Piddington, to designate any considerable extent or area of wind which exhibits *a turning or revolving motion*, without regard to its varying velocity, or to the different names which are often applied to such winds. If used in this sense, it may prevent the confusion which often results from other names, more variable or indeterminate in their signification. Thus, all hurricanes or violent storms may perhaps be considered as cyclones or revolving winds. But it by no means follows that all cyclones are either hurricanes, gales, or storms. For the word is not designed to express the degree of activity or force which may be manifested in the revolving disc or stratum of atmosphere to which it is applied. It often designates light and feeble winds, as well as those which are strong and violent.*

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* As regards the temperate latitudes of the northern hemisphere, the true normal wind is commonly from the southwestern quarter of the horizon; and the accession of a cyclone, except on its right margin, is usually marked by a change of the wind from the western board to some point on the eastern side of the meridian, accompanied and often preceded by a fall of the barometer. On the right margin of its path the cyclone may commence from near the southwest, in perfect continuance with the normal wind with which it here coincides. As the cyclone advances, the wind on the right of its axis-path *veers* "with the sun," or from the east towards the south and west; while on the left side of this path or line the same cyclonic wind changes from the east to the *north*. On and near the axis-path the earlier winds of the cyclone blow *across* this line of progress, from the southeastern quarter to the northwestern, with a falling barometer; and when the axis of the cyclone has passed, its later winds are found crossing this line in the opposite direction, from the northwestern quarter to the southeastern, with a rising barometer.

The true cyclonic wind may not always be found at the earth's surface, in every portion of the path of the cyclone, if its action be feeble, or subject to interruption, It follows that the local directions and changes of the wind in any cyclone, and their effect on the barometer, are much like those exhibited in the gales and storms of the same region, except in the *degree* of their effect; which is doubtless proportioned to the general activity of the rotation, integrally considered.

The cyclones are often productive of rain in a portion of the cyclonic area; but vary in this respect, in different regions, and at different seasons of the year.

UNIVERSALITY OF CYCLONES. — As early as 1833 the author's inquiries led him to announce the conclusion, that the ordinary routine of the winds and weather in these latitudes often corresponds to the phases which are exhibited in the revolving storms, already described, and that a correct opinion, founded upon this resemblance, can often be formed of the approaching changes; and that the variations of the barometer resulting from the mechanical action of circuitous winds and the larger atmospheric eddies pertain, not only to the storms, but to a large portion of the winds in these and the higher latitudes.

The more inert and passive cyclones which seldom gain attention, but which constantly occupy in their transit the greater portion of the earth's surface, appear to move in orbits or courses corresponding with those of the more active class which have been traced on the storm-charts; a result that will not be doubted by those who have given careful attention to this branch of inquiry. In a broad view of the case, the constant succession and progression of the cyclones, in various degrees of activity, constitute the normal condition of the inferior or wind-stratum of the atmosphere, at least in the regions exterior to the trade winds of the globe, to say nothing of their prevalence in the intermediate region, where their presence is shown on some occasions by the most indubitable evidence.

The storm-paths and routes of the cyclones clearly indicate, also, the true course of the principal circulation in the lower atmosphere, on both sides of the equator. An enlarged view of these physical truths

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or to the interposition of bordering winds or cyclones. Even in stormy cyclones, irregularities of direction are often noticed at the surface; but in these cases it commonly happens that the *storm-scud*, at the elevation of a few hundred feet, exhibits locally the true direction of the cyclonic wind. But the changes of direction successively observed in the storm-scud are commonly *in advance* of those in the lowest wind.

and conditions may serve to convince meteorologists and others of the necessity for a thorough revision and correction of the received views of dynamical meteorology.

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6. The Meteorograph, an Automatic Meteorological Register.

By Professor Nathan B. Webster, of Portsmouth, Va.

The object of this communication is to describe a method, simple in its arrangement, and, as far as practically tested, accurate in its results, which shall record the *direction* and *force* of the *wind;* the *hygrometric*, *barometric*, and *thermometric* state of the *atmosphere;* also the *commencement, duration*, and other phenomena of *rain-storms*, as well as the quantity of water precipitated. These indications are for every minute of time, night or day, and will only require the attention of the observer once a week. By reference to the accompanying drawings, it will be observed that the *vane* is attached to a rod (passing through the roof, or, if more convenient, through the top of a box, which may contain the whole apparatus, and be placed on a roof or other suitable spot), and that to the rod is attached a cylinder, which consequently moves with the wind. Around this cylinder is placed a sheet of paper, ruled vertically, to show the points of the compass, and horizontally, to indicate divisions of time. A common or eight-day clock, the weight of which is removed from the case, and by means of fixed pulleys made to descend outside of the case, is so placed that the weight (which should be parallelopiped), and which is prevented from swinging by guide rods, not shown in the drawing, may descend in a path parallel with and near the cylinder. In a tube inserted in this weight is a pencil, kept by a spiral spring gently pressing against the cylinder. Now, as the weight descends uniformly through equal spaces in equal times, it is plain that the intersection of the pencil-marks with the lines on the paper will indicate the direction of the wind during the entire time of the descent of the weight. Sheets taken from such an instrument are here presented, and experience has shown that the appearance of this line will indicate nearly the strength of the wind; thus, in a calm, the line will be *straight* and *uniform*, but *wavy* or *zigzag* according as the tension of the wind varies. This part of the apparatus was in operation about three months, at the Collegiate Institute, Portsmouth, Va., and found to work admirably, without liability to derangement.

Between the clock and the anemograph, place two cylinders vertically, to be turned on their axes by the cord or chain leading from the clock to the weight. The cord or chain makes one turn around the pulley fixed to the end of each cylinder, the circumference of which equals the whole distance through which the weight descends, so that each cylinder makes one revolution during the fall of the weight. Sheets of paper, ruled vertically, to show divisions of time, are placed around these cylinders. A cord sensitive to hygrometric changes is so arranged that a weight carrying a pencil may rise or descend as the quantity of aqueous vapor in the atmosphere varies, at the same time recording such variations as to time and extent.

A funnel placed on the roof conducts the rain-water to a reservoir containing a float, from which rises a stem carrying a pencil for recording phenomena of rains. A siphon barometer, constructed as shown in the drawing, so that a sufficiently large iron float may rise and fall with the mercurial column, furnishes the means of registering the weight of the air.

The thermometer is a compound bar on the principle of Breguet's thermometer, but bent into a spiral, and the interior end made fast to a central pin, so that any variation of the curve must cause a corresponding motion of the external end. A cord attached to the end of the thermometric spiral passes over the barrel of a wheel, while around the periphery of the wheel is wound a cord with the marking weight attached, by this means magnifying the thermal variations. This thermometric spiral may consist of any metals of different expansibility, — as copper and iron.

An arrangement of mirrors may also readily be made to indicate, on chemically prepared paper placed around either of the uniformly rotating cylinders, the times and intensities of solar illumination.

It is not expected that these indications will be as exact as those from *personal observation* with *standard instruments ;* yet, from their continuity and at least relative records, the contriver hopes they may conduce to the advancement of meteorological science.

THE following papers were read, but no copy of them has been furnished for publication: —

7. ILLUSTRATION OF CYCLOIDAL CURVATURE AS INVOLVED IN THE SUPPOSITITIOUS TRAVELLING WHIRLWINDS, AND SOME NEW DEMONSTRATIONS OF THE IMPOSSIBILITY OF STORMS BEING WHIRLWINDS, UNLESS ON A LIMITED SCALE, AS THE CONSEQUENCE OF IN-BLOWING WINDS. By PROFESSOR ROBERT HARE, of Philadelphia.

8. ON THE LAW OF VARIATIONS OF ATMOSPHERIC PRESSURE THROUGH SUCCESSIVE MONTHS OF THE YEAR; AND ITS PRACTICAL APPLICATION TO BAROMETRIC MEASUREMENTS OF HEIGHTS IN THE INTERIOR OF CONTINENTS. By LORIN BLODGET, of Washington.

9. ON THE METEOROLOGICAL PHENOMENA OBSERVED AT VARIOUS POINTS ON THE BOUNDARY SURVEY. By MARINE T. W. CHANDLER, of Philadelphia.

10. A THEORY OF STORMS. By T. BASSNET, of Ottawa, Ill.

11. ON THE PERMANENCE OF THE PRINCIPAL CONDITIONS OF CLIMATE. By LORIN BLODGET, of Washington.

12. THE CLIMATE OF CHILI. By LIEUTENANT J. M. GILLISS, U. S. N.

13. ON THE BAROMETER OFF CAPE HORN. By LIEUTENANT M. F. MAURY, U. S. N.

V. GEODESY AND NAVIGATION.

1. DESCRIPTION OF THE UNITED STATES COAST SURVEY APPARATUS FOR MEASURING BASE LINES. By LIEUTENANT E. B. HUNT, U. S. A., and Assistant in the U. S. Coast Survey.

IN conducting a trigonometrical survey, each connected scheme or system of triangulation involves the lineal measurement of a base line, besides the angular measurement of the vertical and horizontal angles, special to each triangulation station, and the astronomical determinations of latitudes, longitudes, and azimuths. As angular measures can be repeated with great facility, a high accuracy is always attainable with portable instruments, so arranged as to permit the free use of the principle of repetitions. But linear measurements consume so much time, and are so costly, that it is only to a limited extent practicable to repeat extended base-line measures, such as those executed in geodetic operations. Hence the apparatus for base measurements requires to be sufficiently perfect to insure, if possible, from a single measurement, a degree of accuracy corresponding with that resulting from the numerous repetitions of observations by the angle instruments employed. As the apparatus length enters a great number of times in a base line, any error of the apparatus is likely to enter the total base length in a highly magnified form, as also an error of the base length will in exact proportion affect the general geodetic results. As the elements of the earth's figure and magnitude are entirely deduced from the several triangulations executed, and as a very minute accuracy of these elements is essential to the discussion of station errors and local actions, it follows that any serious imperfection of the base apparatus will greatly diminish the general geodetic value of the results and the scope of the local discussions. For these reasons, the requisition of extreme accuracy is eminently rigorous in the construction of a geodetic base apparatus.

The United States Coast Survey extends along the entire Atlantic, Gulf, and Pacific seaboard of our country, and is limited to the narrowest belt which will suffice for the due location of the primary triangles. For this reason, as also to promote the accuracy and facility of its results, our coast has been, since 1844, progressively distributed into eleven sections (nine on the Atlantic and Gulf, and two on the Pacific), in each of which the triangulation proceeds separately, starting from its own base, and, by its connections with the adjoining triangulations, giving and deriving a verification of all prior operations. In two or three of these sections it is almost certain that extra bases will be found necessary or advantageous, and in flat, heavily wooded countries, the primary triangles must be small and numerous, so that accuracy will require some additional bases. Thus it will be seen that certainly eleven, and probably fourteen or more bases, will ultimately have to be measured with the utmost accuracy, as part of the Coast Survey operations. As each of these will be from five to ten miles long, the total length of Coast Survey base-line required will approach near to a hundred miles. Hence it is evident, that, even for economical reasons, the apparatus employed should be as portable and every way as convenient for field use as possible, consistently with its accurate working.

The main essentials for a base measuring apparatus are embraced in the following general conditions: —

1. The extreme points of the apparatus, used as measuring limits, must, under all circumstances of operation, remain at an invariable distance from each other, or the corrections for variations in this distance must be capable of easy and accurate determination.

2. The distance between the measuring limits must be compared with the standard unit of length, to the last degree of attainable accuracy, and its precise length so determined.

3. In the apparatus, the necessary parts and construction for its easy and safe transportation, firm support, accurate contacts, or coincidences, for slope measurements, and also all requisite auxiliaries to the several adjustment manipulations, must be so provided and combined as to give the whole a union of portability, convenience, and delicacy.

The Coast Survey apparatus, as it now stands, was devised with special regard to all the conditions for extreme accuracy and convenience in the field. Some slight modifications, indicated by experience in its use during the measurement of three base lines, have since been embodied, but, in the main, the apparatus remains unaltered from the plans devised by Professor Bache, in 1845, and executed under his direction in 1845 and 1846, by Mr. William Würdeman, then mechanician of the Coast Survey, to whom many of the details of arrangement are due. It may here be stated, that the experience of three base-line measurements with this apparatus has fully shown it to be a highly satisfactory solution of the problem proposed, and it has been found to excel alike in accuracy, economy, and facility of use.

A base line being duly reconnoitred, opened, and graded, and monuments being fixed for the permanent preservation of its extremities, or the base station points, the measurement proper proceeds. The apparatus sent to the field for this purpose, when a primary base is to be finally measured, consists of the following parts: —

1st. Two measuring tubes exactly alike, each being packed for transportation in a wooden box.

2d. Six trestles for supporting and adjusting the tubes, three being fore trestles and three rear trestles, each of which is packed for transportation in a three-sided wooden box.

3d. Eight or more iron foot-plates on which to support the trestles, and a wooden frame is afterwards made to serve as a guide in laying down the foot-plates.

4th. Manipulating rods for the adjustments, a theodolite for making the alignment, and for occasionally referring the tube end to stakes driven for the purpose; also some minor auxiliaries.

5th. A standard six-metre bar of iron in its wooden case arranged for comparisons, and a Saxton's pyrometer arranged for indicating minute variations in length.

The measuring tubes are carefully compared with the standard bar before beginning the measurement, and again after its completion, to make sure of the exact condition of the somewhat complex mechanism in the measuring tubes. For these field comparisons, the pyrometer is simplified by causing the bar or tube undergoing comparison to abut against the spherical head of an arm springing horizontally from the vertical axle, to which is attached the mirror for reflecting the remote arc graduation into the telescope fixed on this arc. Variations or inequalities of the bars examined act on the arm and thus turn the mirror, causing it to reflect the corresponding arc reading into the telescope. A spring is so arranged as to make the arm-head press with a constant pressure against the bar end.

When the comparison with the standard is completed, the foot-plates are successively placed by the aid of the wooden spacing frame, which gives an approximate distance and alignment. Four trestles are so placed and levelled on these plates that the three foot-screws of each rest in the three radial grooves of the plates. The two tubes are then mounted on their trestles, and, the rear extremity having been adjusted vertically over the station-point, the fore tube is then adjusted to make a contact with the rear tube extremity, by means of a level of contact. Both tubes have to be first aligned by the aid of a theodolite advanced some distance on the line, or following the measurement in the field of which two standing sights on each tube are made to cover. The placing of plates proceeds as fast as is necessary for keeping work always provided for the tubes, and, the extra trestles being duly placed and approximately adjusted, the rear tube is carried forward in place, and the adjustments executed. Thus the components of the apparatus are carried from rear to front, in a determined order, and the measuring operations consist in the preparation for, and execution of, these progressive transfers, in effecting the more delicate adjustments, and in making a full record of all the essential circumstances. Points marked on copper nails in the heads of well-driven stakes usually indicate temporarily the end of each day's work, and great care is taken permanently to secure the precise extremity or base-station-point from all disturbance.

From this synopsis of the general character and mode of using the Coast Survey base apparatus, a ready and intelligent transition to the details of its composition and construction can now be made. While many minute arrangements and parts of this apparatus must here be unnoticed, I will endeavor to present a satisfactory summary, such as will enable the members of this Association to inspect with all necessary insight the apparatus itself, which is now open to their examination at the Coast Survey Office.

The tube is a spar-shaped doubled casing, (Fig. 1,) designed especially to embrace and protect the trusses which support and stiffen the system of bars on which the actual measurement depends. Its length is nearly six metres, or about twenty feet. The length of any simple bar of iron, or other metal, is so much affected by variations of its temperature, as to make it necessary, when such bars are used for base measurements, constantly to observe and correct for the temperature, the formula for correction being derived from previous experiments. But changes of temperature cannot, in this case, be exactly determined so as to know at a given moment the precise condition of the bar undergoing change; hence the temperature correction is always uncertain in its value, besides causing much extra labor in observing and reducing. This makes apparent the importance of an arrangement, the limiting points of which will always be found at a constant distance apart under all field circumstances of temperature. No single material can give this exemption from expansions and contractions. It is found, however, in a combination of two metals, having different rates of expansion, and hence admitting a resort to the principle of compensation illustrated in the gridiron or compensating clock pendulum.

This principle was independently applied to the construction of base apparatus composed of one brass and one iron bar, connected by a lever of compensation at their ends, first by Colonel Colby, in the Ordnance Survey of Ireland in 1827 (see Captain Yolland's Lough Foyle Base, p. 10), and again by Mr. Borden of Fall River, in the Massachusetts Survey during the winter of 1830 (see Palfrey's Tables, Survey of Mass., p. 1; Am. Phil. Trans., Vol. IX. p. 34; and N. A. Rev., Oct. 1845, pp. 458–461). Mr. Borden made no provision for causing the two bars in his apparatus to change their temperatures at the same rate, though his tin tubular arrangement admirably checks the frequency and rapidity of such changes as in practice they must undergo. Colonel Colby attempted, by the aid of varnishes and lamp-black coatings, as fixed by numerous experiments in 1827, to make both bars maintain the same temperature during the changes of surrounding heat. He made both bars of the same cross-section, and thought, by regulating the surface radiation and absorption, to effect the required equalization of rate for heating and cooling. His method, though giving a good approximation, is radically faulty, in not taking the specific heats and conducting powers of the bars into the account. The method first introduced and originated by Professor Bache is capable of insuring a very perfect equalization of temperature in the two bars. By numerous experiments in 1845 and 1846, he so arranged the cross-sections of the bars as that, while the two have equal absorbing surface, their masses are inversely as their specific heats, allowance being made for their different conducting powers. Thus, while each receives the same accession of heat in a given time, the temperatures of the two will continue equal, because, except for the conducting rates, their masses are inversely as their specific heats. The same varnish on both surfaces gives them equal absorbent powers. The last minute adjustment of compensation was effected by making one surface slightly more absorbent than the other, as required by circumstances. Thus, as both bars vary essentially together, the point of compensation is never shifted by their diverse actions under thermal variations.

A bar of brass, and a bar of iron, each less than six metres long, are supported parallel to each other, and at one end are so firmly connected together by means of an end block, in which each bar is mortised and strongly screwed, as there to preserve an unalterable relation. The brass bar, which has the largest cross-section, is sustained on rollers, mounted in suspending stirrups, and the iron bar rests on small rollers, which are fastened to the iron bar and run on the brass one. Supporting screws through the sides of the stirrups are adjusted to sustain the bars in place, and also serve to rectify them. Thus, while the two bars are relatively fixed at one end, they are elsewhere free to move, and hence the entire expansions and contractions are manifested at the free end. The medium of connection between the free ends of the two bars is the *lever of compensation*, which is joined to the lower or brass bar by a hinge pin, around which it turns during changes of temperature. A steel plane on the end of the iron bar abuts against an agate knife edge on the inner side of the lever of compensation. This lever terminates in a knife edge turned outward, at such a distance from the centre pin and the other knife-edge bearing, that the end edge will remain unmoved by equal changes of temperature in the two bars. The end edge presses against a steel face in a loop made in the *sliding rod*. This rod slides in a frame fastened to the top of the iron bar, and passes through a spiral spring, which acts with a constant force to press the loop against the knife-edge. The outer end of the sliding rod bears the limiting agate plane. Thus the end agate is not affected in position by the expansions of the brass and iron, acting as they do at proportional distances along the lever of compensation, measured from its sliding end-bearing. The rates of expansion for iron and brass may safely be taken as uniform, between the extreme expansions and contractions to which they are subject in practice, and the compensating adjustment once made is permanent.

The stirrups sustaining the rollers on which the brass bar runs are made fast to the main horizontal sheet of the iron supporting or stiffening work. This consists of a horizontal and a vertical plate of boiler-iron joined, along the middle line of the horizontal sheet, by two angle irons, all being permanently riveted. Circular openings are cut out from both plates to lighten them as much as practicable. A continuous iron tie plate, turned up in a trough form, connects the bottoms of all the stirrups. At the ends, stiffening braces connect the two plates.

We now pass from the compensating to the *sector end* of the tube, at which extremity are arranged the parts giving the readings and for adjusting the contacts between successive tubes in measuring, thus making it the station of the principal observer. The sector end terminates in a *sliding rod*, which slides through two upright bars, and at its outer end bears a blunt agate knife-edge, horizontally arranged, which, in measuring, is brought to abut with a uniform pressure against the limiting agate plane of the compensating end of the previous tube. At its inner end, this sliding rod rests against a cylindrical surface on the upright *lever of contact*, so mounted as at its bottom to turn around a hinge pin. At top this lever rests against a tongue or drop lever, descending from the middle of the *level of contact*, which is mounted on trunnions. The sliding rod, when forced against the side of the lever of contact, presses its top against the tongue of the level, and thus turns the level by overcoming a preponderance of weight given to its farther end, to insure the contact being always at a constant pressure between the agates, the same force being always required to bring the bubble to the centre.

The sector is a solid metal plate, mounted with its centre of motion in the line of the sliding rod, and having its arc graduated from a central zero to the limits of the ascending and descending slopes, on which the apparatus is to be used. A fixed vernier, in contact with the arc, gives the slope readings. A long level and bubble scale is so attached and adjusted to the face of the sector plate, that the zeros of the level and of the limb correspond to the horizontal position of the whole tube. If, then, on slopes the bubble be brought to the middle, by raising or lowering the arc end of the sector (a movement made by a tangent screw, whose milled head projects above the tin case of the tube), the vernier will give the slope at which the tube is inclined, and the slope measure is readily reduced to the horizontal by means of a table prepared for the purpose. The level of contact, and the lever of contact, with their appendages, are all mounted on the sector, and partake of its motions. A knife-edge end of the sliding rod presses on the cylindrical face of the contact lever, this cylinder being concentric with the sector, and the sector can therefore be turned without deranging the contact. In fact, the contacts are made with the sector level horizontal, thus insuring the accuracy of the contact pressure. The contact lever is supported at bottom by two braces dropping down from the sector plate, and a spring acting on a pin in the lever steadies it against an adjusting screw-end. A bracket from the sector plate receives the trunnions of the contact level. A small screw projects from the end of the tube, to clamp or set the lever and level of contact, against a pin in the sector, for security in transportation.

What is called the *fine motion*, required for adjusting the contacts between the successive tubes, is produced by means of a compensating rod or tube, one end of which is attached to the truss-frame by a bracket over the rear trestle, and the other receives a screw terminating in a projecting milled head. This screw turns freely in a collar, bearing, by a projecting arm, against the cross bar which joins the main brass and iron bars, and its nut is the end of the compensation-rod. By turning the screw in one direction, the bars are pushed forward, and the opposite turning permits a spiral spring arranged for the purpose to push back the system of bars, which slides through its supports. Thus the contact is made, by turning the screw until the contact level is horizontal. The compensating-rod is composed of several concentric tubes, alternately of brass and iron, arranged one within the other, and fastened at opposite ends alternately. Thus, when a contact has been made by the fine-motion screw, changes of temperature will not produce derangement, as would be the case if this rod were not compensating. The arrangement permits the observer conveniently to work the fine-motion screw, and to observe its action on the contact level.

The apparatus thus described is inclosed in a double tin tubular case, diaphragms being adapted for supporting and strengthening the whole. The air-chamber between the two cases, 1½ inches apart, is a great check on heat variations. Three side-openings, with tin and glass doors in each tube, permit observations of the parts and of inserted thermometers. The ends are closed, only the sliding rod ends projecting at each extremity and exposing the agates. In transportation, brass guard-tubes protect these, and tin conical caps are screwed on the tube ends. The fine-motion screw, the sector tangent screw, and the contact lever clamp screw, project beyond the case. The tube is painted white, which, with the air-chamber and the thorough compensation, effectually obviates all need of a screen from the sunshine, which has usually been deemed a requisite.

The tube rests on a fore trestle and rear trestle, which are alike, except in the heads. Each trestle has three legs, composed of one iron cylinder moving in another by means of a rack, pinions, and crank, so as to raise or sink the head-plate. The levelling and finer adjustment is by means of a foot-screw in each leg, by working which a circular level on the connecting frame is adjusted. A large axis screw, resting on the connecting frame, and rising within a tubular nut, is turned by bevelled pinions, worked by a crank, and thus raises and lowers this tubular nut, and the cap-piece which it supports at top. The axis screw, the leg racks, and the foot-screws give three vertical movements in the trestle, by which its capacity for slope measurements is much amplified.

In the cap of the rear trestle, a lateral and a longitudinal motion are provided for, by means of two tablets arranged to slide, the upper one longitudinally on the lower one, and the lower laterally on the head-plate of the axis screw tube. Long adjusting screw-handles extend to the observer's stand from these two plates, and from the axis screw, enabling him to raise or lower, to slide forward or back, to the right or the left, the rear end of the tube. The fore trestle is similar, except that its head is only arranged for a lateral movement, and a second observer makes its adjustment by a simple crank.

Four men can carry a tube, by levers passed through staples in blocks, strapped under the tubes. The principal observer and an assistant make the contacts and rectifications; the first assistant directs the forward tube, and another preserves the alignment with a theodolite. A careful recorder notes down the observations, and an intelligent aid places the trestles and foot-plates. The labor of grading, especially in level sand lines, is quite trifling.

The first base line measured with the apparatus now described was the Dauphine Island Base, near Mobile, seven miles long, which was measured by Professor Bache in 1847. (C. S. Report, 1847, p. 39.) The party was on the ground six weeks, between April 30th and June 12th, though only seventeen working-days were consumed in the final measurement. The greatest day's work was 183 tubes, or near seven tenths of a mile. From some remeasurements, the greatest supposable error for the entire base was computed to be less than six tenths of an inch.

The second base measurement with this apparatus was also by Professor Bache, in 1848 (C. S. Reports, 1848, p. 43, and 1849, p. 38), being the Bodies' Island Base, N. C., of about $6\frac{3}{4}$ miles long. Ten working-days were employed in the actual measurement, between the 4th and 23d of November. The greatest day's work was 1,692 metres, or 1.06 miles in 8½ hours. Several partial remeasurements gave the total probable error for the entire base at less than one tenth of an inch, and the greatest supposable error at less than three tenths of an inch.

The only other base hitherto measured with these means is the Edisto Island Base, S. C., which operation was conducted by Professor Bache, between the 3d and 18th of January, 1850 (C. S. Report, 1850, p. 34), thirteen days being occupied in the actual measurement. Its length is about 6⅔ miles, and it was much more uneven than the previous base. The greatest day's work was 1.122 metres, or about three quarters of a mile. A partial measurement gave one tenth of an inch as the probable accidental error of measurement for the whole base.

It will be abundantly evident, on examining the results of other modes of measurement, that the Coast Survey apparatus is a superior combination of the requisite elements for such operations, giving a gain in accuracy, rapidity, and economy of use over its predecessors. The multiplication of bases is no longer a source of such formidable expenditures of time and money; hence, geodetic operations are much facilitated and benefited by this fundamental improvement in the instruments employed. The more perfect compensation from regulating the masses of the bars, the application of the principle of contact, indicated by the level, the stiffness of the support for the bar system, the sector for slope measurements, and the trestles combining such a variety of movements with very great firmness,— these features all attest the thorough study of the problem which was made by Professor Bache, preparatory to calling forth the peculiar skill of Mr. Wurdeman. Bessel's contact level, before employed in the comparison of standards, has the same readily available accuracy in this apparatus, and should supersede the comparing microscope entirely for final measurements. For field comparisons with standards, the peculiarly elegant principle of Saxton's pyrometer is even better than the contact level.

Whatever improvements may yet need to be made in base measuring apparatus, this important point is now reached, that the bases are measured at once, with an accuracy far exceeding that of the angular measures given by any practicable number of repetitions on portable angle instruments, and of the same order with the comparisons between the actual standards and their copies used in the measurements.

THE following papers were read, but no copy of them has been furnished for publication: —

2. THE APPLICATION OF THE GNOMONIC PROJECTION TO NAUTICAL CHARTS, SHOWING BY INSPECTION THE GREAT-CIRCLE COURSES AS STRAIGHT LINES, IN THE SAME MANNER AS THE RHUMB-LINES ARE MADE TO APPEAR AS STRAIGHT LINES ON THE MERCATOR'S PROJECTION. By COMMANDER C. H. DAVIS, U. S. N.
3. ON A NEW INSTRUMENT FOR FACILITATING THE PROJECTION OF GREAT-CIRCLE ROUTES IN CHARTS, AND FINDING BY INSPECTION THE COURSE AND DISTANCE. By PROFESSOR W. CHAUVENET, Annapolis, Md.

B. CHEMISTRY, NATURAL HISTORY, AND GEOLOGY.

I. CHEMISTRY.

1. ILLUSTRATIONS OF CHEMICAL HOMOLOGY. By T. STERRY HUNT, of the Geological Commission of Canada.

THE corresponding species of homologous genera have similar chemical functions, and are subject to like modes of decomposition, yielding new species which are themselves homologous. The susceptibility of particular modes of decomposition, and the strength of affinities, vary like the physical characters, as we rise or descend in a homologous series, so that relations which are very apparent between approximate genera become less evident when those more remote are compared. According to Gerhardt, who first drew attention to the study of these relations, the formulas of homologous genera always differ by $n\,(C_2M_2)$, the oxygen, nitrogen, &c. remaining the same. He thus restricted homology to carbonaceous species; but to these may be added hydrogen, water and ammonia, which, as I have elsewhere shown, are both the prototypes of all carbon species, and the first terms of three important homologous series.* Isomeric species, which have the same composition and equivalent weight, but differ in chemical and physical characters, constitute parallel homologous series, represented by one general formula.

In the normal species, M represents hydrogen, which, in the other species, is wholly or in part replaced by a metal, or by chlorine, bromine, or iodine. Nitrogen may also be said to replace hydrogen as in prussic acid, C_2HN, and in acetonitryl, C_4H_3N, and its homologues, which may be supposed to correspond to C_2H_2 and C_4H_4. The first of these azotized species is the only one which exhibits acid characters; the others are as neutral as the hydrocarbons. There are perhaps two series with the formula $n\,(C_2M_2)$, one corresponding to acetonitryl, which includes olefiant gas and its homologues, and another, which, like hydrocyanic acid, can exchange an equivalent of hydrogen for a metal, and which will be to the acetene series $n\,(C_2M_2) + H_2$ what aldehyde is to alcohol. These azotized species are subject to peculiar modes of decomposition, and must be regarded as aberrant members of their respective genera.

The species in which nitrous acid is said to replace hydrogen offer a peculiar case. While their characters show them to belong to the same type as the normal species, they differ by the addition of O_4, and by containing N in the place of H, so that it is only by assuming HNO_4 to be the equivalent of H_2, that the nitric species can be reduced to the generic formula. There are other instances of species which closely resemble each other in chemical and physical characters, but differ in the amount of oxygen; the bodies of the benzoic and salicylic series, which differ by O_2, the malic and tartaric acids, and the alkaloids quinine and cinchonine, whose formulas exhibit the same difference, are examples.

The ammonia salts of monobasic acids contain NH_4 instead of H, and, admitting the nitrogen to replace the equivalent of hydrogen, differ from the normal species by H_4. Analogous to this is the difference between piperidine ($C_{10}H_{11}N$) and its homologues arsine and stibethine, n (C_2M_2) + HN, (which may be regarded as aldehyde-ammonias,) and the alcohol-ammonias, n (C_2M_2) H_3N.[*] The resemblance between piperidine and the latter bases is very close. The aldehydes and acids of the acetic and acrylic series differ in the same way by H_2. The results of decomposition in bodies thus unlike are necessarily different, but we must nevertheless admit a certain homology between species differing by n H_2 and n O_2.

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* American Journal of Science, New Series, Vol. XVII. p. 194.

From the resemblances between the sulphites and carbonates we are led to conclude that carbon may replace sulphur and its congener, oxygen, as nitrogen does hydrogen. In this way water and metallic oxides, O_2M_2, correspond to the cyanides, C_2MN, as well as to the hydrocarbons, C_2H_2, while peroxide of hydrogen, O_4H_2, may be compared with cyanogen, C_4N_2, and with nitric oxide, O_4N_2, and nitrous acid, O_4NH, with the cyanic acids, (C_2O_2) NH and (C_2S_2) NH.[†]

The relation between C_2M_2 and O_2M_2 is exemplified in the metallic salts. The three nitrates of lead in the ordinary notation are represented by NO_5, PbO, NO_5, 2PbO, HO, and NO_5, 4PbO, HO. Excluding the water, and doubling these formulas, we may write them (Pb_2O_2), N_2O_{10}, $(Pb_2O_2)_2$, N_2O_{10}, and $(Pb_2O_2)_4$, N_2O_{10}, the common formula for the nitrates of lead being $(Pb_2O_2)_n$, N_2O_{10}. The different acetates of the same metal are represented by $(Pb_2O_2)_n$, $C_{16}H_{12}O_{12}$ (the ordinary formula of the neutral acetate $C_4H_3PbO_4$ being multiplied by four), and the different values of n are 2, 3, 6, and 12. The various basic sulphates, nitrates, and chlorides afford similar series of salts, which exhibit in their physical characters chemical functions, and modes of decomposition, relations completely analogous to the homologous series of Gerhardt. From the relations which Graham has shown to exist between surbasic and hydrated salts, a similar view must be extended to the latter, and we conclude that species differing by *n* (O_2M_2) and *n* (O_2H_2) may be homologous.*

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[*] American Journal of Science, Vol. XIII. p. 206.

[†] In this way nitrobenzene may be compared with bitter-almond oil, $C_{12}H_5NO_4$ = ($C_{12}O_2$) (H_5N) O_2, which corresponds to $C_{14}H_6O_2$. The nitroprussids of Playfair, whose analyses, as well as those of Kyd, have been shown by Gerhardt to agree best with the formula $C_{10}Fe_2M_2N_6O_2$, may be looked upon as ferrocyanides, which, having lost M_2, have become bibasic, and then, under the influence of nitric oxide, have exchanged C_2N for its equivalent O_2N. The formula of these salts then becomes ($C_{10}O_2$) $Fe_2M_2N_6$, corresponding to $C_{12}Fe_2M_2N_6$. See American Journal of Science, Vol. XV. p. 151.

In determining the equivalent weights and homologous relations of soluble and volatile species, we are guided by their chemical relations, the results of their partial decomposition, and in many cases by their boiling points and the density of their vapors; but fixed insoluble mineral species, such as the oxides, silicates, and carbon-spars, furnish no such data. We have, however, their density, the results of their ultimate analysis, and their crystalline form, considered in connection with those predominant cleavages and secondary forms which constitute the *characteristic* of a natural group; to these may be occasionally added color, lustre, and hardness.

Dimorphous species generally differ in hardness, density, and color, and sometimes in chemical characters, as in the case of calcite and aragonite, rutile and brookite, diamond and graphite. From the fact that aragonite is by heat changed into calcite with increase of volume, and from some other considerations, it is probable that there is a greater condensation in prismatic than in monometric or rhombohedral crystals. But as there is every reason to suppose that the real equivalents of most species are multiples of those deduced from their empirical formulas, the variations in the density of dimorphous species may be in part due to differences in their equivalent weights. This is doubtless true in the case of rutile and anatase, which, while they differ in density, and in their conduct when ignited, are both pure oxide of titanium, and are homœomorphous.

I have elsewhere endeavored to show that the law of equivalent volumes is strictly applicable to all homœomorphous species.† The considerable variations which appear in the volumes corresponding to the ordinary empirical formulas were supposed by Kopp to be connected with the differences in angle met with in homœomorphous species. But similar variations occur in related monometric species, as the alkaline chlorides, and lead us to conclude that in both cases the real equivalent weights are various multiples of those deduced from the empirical formulas, and correspond to different homologous genera. The density then varies directly as the equivalent weight among homœomorphous species; but as the density of the same body may vary in different crystalline systems, we cannot from their densities determine with certainty the comparative equivalent weights (and consequently the homologous relations) of species dissimilar in crystallization. Thus, while from its specific gravity we conclude that the equivalent weight of rutile is greater than that of anatase, (the two being homœomorphous,) we cannot determine the relation of the equivalents of these species to that of the trimetric form brookite. Hence, in the study of fixed insoluble bodies, whose equivalent weights cannot be determined by independent means, only homœomorphous species should be compared.

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* These views have already been expressed in the American Journal of Science, Vols. XV. p. 230, XVI. p. 204, and XVII. p. 194.

† American Journal of Science, XVI. p. 203.

In the paper just cited, I have written the formula of alumina, $\mathrm{alO} = (\mathrm{Al_2O_3} \div 3)$, and silica, SiO (having an equivalent weight of $7.1 + 8.0 = 15.1$). Similar formulas may be extended to the analogous boracic, titanic, columbic (niobic), and tantalic acids. I have there insisted upon the fact, that the proportion of silica may vary considerably in the different genera of a natural order, and even among different species of the same genus. This is seen in the different staurotides and kyanites, and in the hornblendes, diopside, and the aluminous pyroxenes, like hudsonite, in which the oxygen ratios of the silica and bases are respectively 18 : 8, 16 : 8, and 9 : 8. This variation is well illustrated in the triclinic feldspars; these constitute a genus, of which albite may be taken as one representative and anorthite as the other. Anorthite, in its purest form, is a lime-feldspar, and the results of its analysis are well represented by the admitted formula, $\dot{\mathrm{Ca}}_3\,\dddot{\mathrm{Si}} + 3\,\dddot{Al}\,\dddot{\mathrm{Si}}$, which requires silica 43.2, alumina 36.7, lime 20.1 = 100.0. Albite is as truly a soda-feldspar, and the ordinary formula, $\dot{\mathrm{Na}}\,\dddot{\mathrm{Si}} + \dddot{Al}\,\dddot{\mathrm{Si}}_3$, requires silica 68.7, alumina 19.5, soda 11.8 = 100.0. In order to show the chemical relation of the two species, it is necessary to reduce them to a common formula, but there is no direct means of fixing their real equivalent weights. The volumes of different prismatic species vary: while piperine has a volume of 468, and the tribasic phosphates and arseniates of soda, together with lactose and yellow prussiate of potash, give from 230 to 235 (or perhaps some multiple of this number, and even of that of piperine), other species give 430, and natron and the hydrated sulphate of alumina keramohalite 199.6 and 201.0, or perhaps double these, in round numbers about 400. For comparison between each other, we may assume the latter equivalent volume for the feldspars.

If we multiply the above formula of albite by four, and that of anorthite by $\frac{8}{3}$, and expand the two, we have the following formulas and equivalent weights, to which the densities and equivalent volumes are affixed : —

Albite $(Si_{48}al_{12}Na_4)\ O_{64} = 1054.4 \div 2.62 = 402.4.$
Anorthite $(Si_{32}al_{24}Ca_8)\ O_{64} = 1118.4 \div 2.76 = 405.0.$

Between these two species may be arranged oligoclase, andesine, and labradorite. By comparing the analyses of these feldspars, it will be seen, that, as we descend the series, lime replaces soda in an increasing proportion, and the sum of these two bases becomes greater. At the same time, the amount of alumina increases, and with it the density, so that they may all be represented by $M_{64}O_{64}$, and the volumes calculated from their recorded densities vary only from 399.2 to 405.0. The formula of labradorite, for instance, may be written

$$(Si_{38}al_{19}Ca_5Na_2)\ O_{64} = 1090.4 \div 2.70 = 404.0.$$

This requires silica 52.1, alumina 29.6, lime 12.7, soda 5.6 = 100.0, and corresponds to the analyses of the labradorite from Egersund, Faroë, Dalarne, and Glasgow; the labradorites of other localities lead to different formulas, and the various analyses of oligoclase are by no means accordant. Petalite, which may be regarded as a lithia-feldspar, enters into the same general formula, and is represented by

$$(Si_{51}al_{10}Si_3)\ O_{64} = 983.9 \div 2.45 = 401.5,$$

requiring silica 78.27, alumina 17.31, lithia 4.42 = 100.00. The monoclinic feldspar, orthoclase, is homœomorphous with albite, and is generally regarded as a potash-albite. It is distinguished by its lower density. An albite containing potash in the place of soda would have the same equivalent weight as anorthite, and might therefore be expected to possess the same density, while the observed specific gravity of orthoclase is between 2.50 and 2.60. This feldspar, then, does not enter into the same genus as the preceding, and probably belongs to a homologous type $M_{60}O_{60}$. The two formulas which most nearly accord with the different analyses of orthoclase are the following : —

$$(Si_{44}al_{12}K_4)\ O_{60} = 1058.4 \div 2.60 = 407.0.$$
$$(Si_{45}al_{12}K_3)\ O_{60} = 1026.7 \div 2.55 = 402.6.$$

The first requires silica 62.77, alumina 19.43, potash 17.80; and the second, silica 66.18, alumina 20.02, potash 13.80.

It is impossible to say how far the feldspars which we have placed between albite and anorthite are to be regarded as distinct species, or as mixtures. It is well known that closely related and homœomorphous species crystallize together in proportions which seem subordinate to no simple ratio, as in the case of the hydrated sulphates of copper, iron, cobalt, manganese, zinc, and magnesia, the chloride and iodide of potassium, and the mixed alums, in which ferricum, chromium, and ammonium partially replace aluminum and potassium; these mixed crystals may perhaps be looked upon as mechanical aggregates of the different species. Heintz has found by fractional precipitation of their solutions, that mixtures of homologous fatty acids, which cannot be separated by crystallization, have hitherto been mistaken for distinct species; thus he has shown the so-called margaric acid (C_{34}) to be a mixture of stearophanic acid (C_{36}) and palmitic acid (C_{32}). In like manner many, if not all, of these intermediate feldspars, are probably mixtures of the two typical species, albite and anorthite. Even these are not always free from a similar source of impurity; the lime, sometimes amounting to two per cent. in the former, indicates an admixture of a lime-albite, and the small and variable amount of potash in this and the other soda-feldspars points to an associated potash species, while the soda and magnesia found in anorthite indicate that this mineral is rarely free from an admixture of a soda or magnesia feldspar. The same may be said of the small portion of soda usually found in petalite.*

The greater number of analyses of orthoclase may be referred to the second formula given above, with an occasional mixture of a lime-feldspar, and perhaps of a potash-feldspar, with the first formula. Many varieties of this feldspar contain a large amount of soda, and a glassy feldspar from the Drachenfels, analyzed by Schnederman, appears to have been a nearly pure soda-orthoclase. The different scapolites may, in a similar manner, be referred to a few species whose mixture has produced the variations met with in analysis. The possibility of such mixtures of related species should always be kept in view in the study of mineralogical chemistry.

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* Since writing this paper I find that M. Scheerer has lately proposed to regard the feldspars as combinations of anorthite with labradorite, albite, or orthoclase, or of labradorite with albite. (Pogg. Ann. LXXXIX. 19, cited in Liebig and Kopp's Jahresbericht for 1853, p. 805.) According to Dana, he has described a certain feldspar as consisting of one equivalent of oligoclase, one of albite, and one of orthoclase, and another of one of oligoclase and two of orthoclase. From this it would seem that Scheerer, faithful to dualistic traditions, entertains a view analogous to that advanced by Hermann with regard to the composition of the epidotes, (Jour. f. prakt. Chem., T. XLIII. pp. 35 and 81,) and considers these feldspars as chemical unions, instead of mixtures of the different species; but I have not seen the original memoir. The views of this distinguished chemist are, however, in some important respects, an anticipation of my own.

The relation between the formulas of albite and orthoclase is analogous to that between the hydrocarbons $n\ (C_2H_2)$ and the carbon-spars $n\ (C_2M_2O_6)$; it is what we have elsewhere called a homology of the first kind, represented by $n\ (M_2O_2)$. To this type, which is also that of the spinels, all silicates may be referred, except a small number, which, like eudialyte, sodalite, and pyrosmalite, contain metallic chlorides, haüyne, nosean, and lapis-lazuli, which contain sulphates, and cancrinite, which holds a portion of carbonate. These are respectively basic chlorides, sulphates, and carbonates, and are represented by $(M_2O_2)_n$, MCl, $(M_2O_2)_n$, $S_2M_2O_8$, and $(M_2O_2)_n$, $C_2M_2O_6$. Hermann's recent analyses of some scapolites show a composition analogous to cancrinite.*

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* The homology between species differing by $n\ (M_2O_2)$, and the substitution of water for a metallic oxide in the formulas of species thus related, is illustrated in some homœomorphous minerals noticed by Dana; thus analcime is essentially a hydrated leucite, and ittnerite a hydrated sodalite; the monoclinic heulandite has the formula of orthoclase with the addition of water, while pectolite and laumontite have the form, and, excluding the water, the general formula of augite. (See Dana's Mineralogy, 4th ed., Vol. II. p. 302.) While admitting the claims of minerals thus related to the rank of distinct species, Mr. Dana rejects the hydrous iolites and many hydrous pyroxenes, as being products of alteration, and regards malacone and œrstedite as results of the hydratation of zircon, while the hydrous minerals atheriasite and algerite are in the same way set aside as altered scapolites; the hydrous kyanites, orthites, and triclinic feldspars are viewed in a similar manner, and only the evident aqueous origin of the zeolites above named appears to save them from a similar fate. May we not rather ask, in view of these relations, whether it is not possible that many more of these minerals may have originally crystallized in a hydrated state, and whether the great variations observed in the composition of the different scapolites of Arendal and Bolton may not have existed from the formation of the rock, and point to distinct species or mixtures of species, as in the case of the different feldspars found associated in some granites?

Muspratt has shown a parallelism in constitution and a homœomorphism between the sulphites $S_2M_2O_6$ and the carbonates $C_2M_2O_6$. In connection with this, as an instance of the resemblances already pointed out between species differing by O_2, we may notice the relation between the carbonates and the sulphates $S_2M_2O_8$. Hausmann has remarked the general homœomorphism between the carbonate and sulphate of lead; and the close likeness of form, which, according to Dana,* is observed in three crystalline systems, between the sulphates dreelite, anglesite, and glauberite, and the sulphato-carbonates susannite, leadhillite, and lanarkite, confirms the view of their homology.

Passing now to the titanates, we find that the titaniferous iron oxides, which crystallize either in the monometric or rhombohedral system, may contain very variable amounts of titanic acid, and are represented in the formulas of Mosander as compounds of titanate of protoxide of iron TiO_2, FeO, with very variable amounts of peroxide of iron. It is not worth while to inquire whether the titanate of iron should be represented as above, or whether, as Rose maintains, it is Ti_2O_3, Fe_2O_3, a combination of two sesquioxides, although we may remark that the latter view would hardly be extended to perofskite TiO_2, CaO, which is monometric, and has the same volume as iserine. Hypotheses such as these have properly no place in a science which looks upon a species as an individual, in which those species which may be obtained from its decomposition have no actual existence. The different titaniferous irons, having the same equivalent volume, exhibit in the varying portions of titanic acid and iron oxides differences such as we have seen in the pyroxenes and feldspars, and may be referred in our notation to a common formula n $(TiFe\,fe)_2\,O_2$ (titanic acid = Ti_2O_2), corresponding to magnetite, martite spinel, pitchblende, pyrrhite, pyrochlore, and perofskite, to which we may add boracite; and, in addition to these anhydrous oxides, the hydrated oxide of lead $(Pb_3H)\,O_4$, which, according to Payen, crystallizes in regular octahedrons.

Among rhombohedral species we ascend through the hydrous oxides brucite, red-zinc-ore, and corundum, to the silicates chabazite, dioptase, willemite, phenakite, and beryl, all belonging to one homœomorphous group, and through these species, in which the amount of silica increases, connected with quartz.

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* American Journal of Science, XVII. p. 210.

In the dimetric system we find titanic acid as rutile and anatase, with which we place cassiterite and œrstedite; this last, a titanate of zirconia with some silica constitutes a transition from rutile to zircon, which is a pure silicate. These form a homœomorphous group, to which may be referred hausmannite $(Mn\,mn_3)O_4$, corresponding to magnetite. To another homœomorphous dimetric group belongs braunite, a sesquioxide of manganese (mn_2O_2), corresponding to martite and ologiste, and the tungstates of lead and lime $(W_3M)\,O_4$, with the corresponding molybdate of lead, and the tantalate fergusonite $(Ta_2\,(Yt\,Ce)_4)\,O_6$.

Azorite, a columbate of lime, approaches rutile in form, while the trimetric brookite is homœomorphous with columbite, ferro-tantalite (including cassitero-tantalite), samarskite, mengite, and wolfram, from which we pass to polymignyte, polycrase, æschynite, wöhlerite, and euxenite, which are homœomorphous, and at the same time somewhat related in form to the previous group. In the one we have titanic acid and titanates, tantalates, stanno-tantalates, columbates, and tungstates, with very variable proportions of acid and base, and in the other titanates, and tantalates associated both with titanic and silicic acids. To these last we add pyrolusite and polianite, $Mn_2\,O_4$, and the hydrous species, göthite and diaspore $(fe_3H)\,O_4$, and $(al_3H)\,O_4$, while manganite $(mn_3H)\,O_4$ is homœomorphous with brookite. Valentinite, sb_2O_2, belongs to this group, and perhaps also the trimetric red-copper-ore observed by Kenngott.*

By these homœomorphous relations we bring together anhydrous protoxides, hydroxides, sesquioxides, and oxides, like MnO_2 and TiO_2, and pass from the last to titanates, with varying proportions of base, and thence through species in which silicon replaces a portion of titanium, to silicates like zircon. We find homœomorphous tantalates with various amounts of base, related to others containing portions of titanic, stannic, and silicic acids, connecting in this manner the salts of these different acids with one another. When in this connection we consider the relations between silicates, like the different feldspars and pyroxenes, we are led to conclude that the distinctions of acid and base have no value beyond the fixing of species, and must be lost sight of, in a general system which shall reduce all these species to the common type already indicated.

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* In tracing out these crystallographic relations, we are entirely indebted to Mr. Dana, whose admirable investigations on the subject of homœomorphism will be found embodied in the fourth edition of his System of Mineralogy, Vol. I. pp. 189 – 210.

No mineralogical classification can be complete which does not take into account both the chemical and physical characters of species; and the connection between these, which is shown in the relation of equivalent weight to specific gravity, must constitute an important element in a natural system. Guided by their physical characters and composition, we bring together such homœomorphous species as belong to one chemical sub-type, and, from the densities, fix their formulas and comparative equivalent weights. From the comparison of formulas and the associations of these different minerals, we must also decide which are to be considered as mixtures, and which true species. Until we shall have determined, with certainty, the comparative volumes of dissimilar crystalline forms, the relations of species differing in this respect must be decided by their general affinities, and their places in a homologous series remain undetermined. In this way we may hope to arrive at a mineralogical classification, which shall satisfy alike the chemist and the naturalist.

THE following papers were read, but no copy of them has been furnished for publication: —

2. ON THE VOLUMETRIC DETERMINATION OF NITRIC, ARSENIC, ANTIMONIC, AND STANNIC ACIDS, AND ON THE SEPARATION OF MANGANESE, COBALT, AND NICKEL. By DR. WOLCOTT GIBBS, of New York.

3. ON THE METEORIC STONES; WITH AN ACCOUNT OF SOME RECENTLY DISCOVERED. BY PROFESSOR J. LAWRENCE SMITH, of Louisville, Ky.

4. ON THE USE OF HYDROGEN GAS TO DISPLACE SULPHURETTED HYDROGEN IN THE ANALYSIS OF MINERAL WATERS. By PROFESSORS W. B. ROGERS and R. E. ROGERS.

5. Decomposition of Water at the Ordinary Temperature, by an Alloy of Zinc and Antimony; with a Description of a New Process for procuring Hydrogen. By Professor J. P. Cooke, Jr., of Cambridge.

6. On two New General Methods of Chemical Analysis. By Dr. Wolcott Gibbs, of New York.

7. A New Filtering Apparatus. By Professor J. P. Cooke, Jr., of Cambridge.

8. Researches on Arseniated and Antimoniated Hydrogen, and their Relations to Toxicology. By Professor Raphael Napoli, of Naples.

9. On the Chemical Relations of Odors, and their Employment as Tests. By Geo. C. Schæffer, of Lebanon, Ky.

II. NATURAL HISTORY.

1. Experimental Observations on Taste and Smell. By Dr. Theodore C. Hilgard, of Philadelphia.

Our conceptions being dependent on the material afforded to the mind by the sensations, we should endeavor to increase our knowledge, our judgment, and the number and distinctness of our sensuous discriminations, by a special study of their qualitative nature in the first place, and secondly, of the conditions under which we receive them, in order rightly to appreciate what part is taken by *immediate consciousness*, and what part by *judgment;* the latter being liable to error at each instance it enters into the combination.

Best of all have been hitherto understood both the organic and outward conditions relating to the mechanism of *eyesight*, and next of *hearing*. The remaining organs and their sensations, not relating as much as the two former to the social and intellectual necessities of man, but rather to those of individual existence, have been very much neglected, so that, when they are called into action simultaneously, their very nature has been abundantly confounded, and compounds of several sensations have been spoken of as simples.

Thus many cutaneous perceptions have been accounted as *specific* sensations of the oral and of the nasal cavities, simply because they are perceived by the same organs as the specific ones.

Sensations of sight are not liable to be confounded with those of hearing, and hence our terms for either are unequivocal. But as for the remaining senses we have not even so much as entirely discriminative and exclusive names for each of them. *Pungency*, *acridity*, and *acerbity*, or astringency, must surely be considered as cutaneous sensations of the mucous membranes, the former being identical with a sudden and transient irritation; the second is a slight but continued one; and the third a feeling of shrivelling, or contraction of the tissues, as it is frequently experienced at the points of the fingers. The feelings of touch, of temperature, and of pain are undoubtedly distinct from one another, though accounted identical by a distinguished authority, to suit a theory that each nerve conveyed only one kind of perception, which, nevertheless, might be true. Touch, as well as temperature, can gradually merge into *pain*, by applying the outward excitants too strongly; but this only shows *est modus in rebus*, and that on every transgression pain or destruction will ensue. An impression of sound or light heightened above the measure will also produce pain, but probably rather by a simultaneous excitement of nerves conveying pain, than by a transformation of another sensation *into* pain. If we taste a diluted acid, we shall experience little beyond acidity. But if it be more concentrated, the sensation of sour will be almost altogether overpowered by another of burning pain and great astringency (shrivelling), from the action on the substance, which it will also destroy, if continued.

It will be seen that what we call *smell* comprises the specific sense of the nasal organ and the common cutaneous sense thereof; and what we call *taste* comprises, besides a very delicate cutaneous sense, two senses entirely different from each other, one being the specific sense of the oral cavity, and the other a simultaneous action of the nasal organ.

Many things " taste as they smell." From this, and the fact that, in certain catarrhal affections of the nose *tastes* become " dull," the physiologist above referred to was led to say, that there seemed to be a sort of *resemblance*, or even *transition*, between smell and taste, although at their extremities they were evidently very different. Others concluded that olfactory nerves were commingled with those of the tongue from the very origin in the brains, that *terra incognita*, — an explanation as fallacious as the other.

We had classified *tastes* into two groups: those known by the names of sour, sweet, salt, and bitter, on the one hand, and on the other, as quite a *sui-generis* class, those "*characteristic*," "*peculiar*," or "*flavored*" tastes, offering as common characteristics, in the first place, sensations widely different from those named above, but similar among each other, all having, as it were, a sort of "volatile" character, being very delicate and instantaneous, yet so transient as to require a constant repetition, which could not be produced by an act of volition as it seemed, but, on the contrary, rather evaded a close observation, disappearing almost as soon as particular attention would be directed on them. These comprise, also, all those tastes that "are like smells," with which they enjoy also the common appellations of "flavor," "aroma," and (in German) "Duft." This class comprised all the *aromatic* tastes, the characteristic tastes of fruits, the tastes of meat, of baked, of boiled, of green substances, — even such "insipid" tastes as of boiled potatoes, starch, solution of gum-arabic, namely, their *characteristic* flavors.

Being told (in 1849) of a remark of Professor Semple, of Philadelphia, that *camphor* would not be tasted while pinching the nose (so as to close it), and that perhaps it would be worth while to try it on other substances, on repeating the experiment, I found what I had anticipated, namely, that it was only the camphor taste *so called* that would disappear, while the bitterness of that substance was perceived only the better for being isolated.

At once the idea suggested itself to me, that the same suppression would be observed on the whole *class* I had called pure flavors, to which, of course, the camphor-flavor, too, belonged; in fact, *on all the so-called characteristic tastes*. And so our comprehensive experiments proved it to be. The characteristic tastes would invariably disappear, leaving only a "dull" one, and that permitted of no nicety in discriminating victuals or other substances by taste.

Now what might be the cause? The most obvious effect of pinching the nose is, to disconnect the air contained within from that without; and as the most obvious consequence of this it would appear that the air within *cannot move then!* At once the idea suggested itself, that it required *a rush of air from within to perceive those delicate or fine tastes or flavors*, as it takes a rush of air from without into the nose to perceive what we call a smell; that the two were of *one and the same nature*, and that they must be *perceived in the same organ*, which is the upper part of *the intricacies of the nose.* The sail-shaped expanse of the upper conch would readily receive all the air pushed up by the motions of the tongue, thus reaching immediately the very region of the distribution of the olfactory nerve, or first pair, which at once accounted for the more distinct and comprehensive sensations excited by odorous or even inodorous substances, when placed on the back part of the tongue. Thus, rose-leaves, when masticated, will, beside their sugar taste and smell of roses, yield an unpleasant, mouldy one of vegetable tissue (together with the astringent action on the induments of the tongue). The air entering by the atmospheric pressure, and by rarefaction of the air within, through the nostrils, will chiefly follow the wide lower channel, and enter the upper convolution rather by diffusion than directly, thereby giving a chance of perception only to substances originally less rarefied than they need be when coming from within. Therefore, penetrating smells, those that quickly diffuse, are more readily perceived, and often, as in ammonia, with very unexpected rapidity, reaching the nervous expanse suddenly, in the midst of inhalation of a current that at first seemed harmless. Such smells are liable to be reperceived after many hours, an uncommon motion of the head giving them a free passage from some sinuosity they had impregnated by diffusion, on a sensitive part of the membrane, where they are reperceived.

In confirmation of this view, I had observed that fine flavors, of wine, for example, are tested with lips quickly closed, and the tongue performing repeatedly a sort of peristaltic motion, moving backward along the palate, and ending with a sort of semi-deglutition, in which instant the flavor is most strongly perceived, the airy contents of the oral cavity being then most completely pushed up into the nasal cavity, — which proceeding is interrupted by any person in whom a strong attention is excited, whence those flavors *elude* a close attention, — and in a similar way, but by *anti*-peristaltic motion, ructus of air from the stomach will convey the exact "specific" tastes of some kinds of food hours after the meals have been taken. The oral sensations proper are generally examined with lips open, so as to give escape to the flavored air, while the tongue, retaining in the velvety coating of its surface all the soluble substances dissolved in the saliva, keeps moving along the palate, whose rippled surface serves to brush the fluid from one part of the tongue to another.

As a definite proof of my theory, it was necessary to devise a means of separating the oral cavity from that of the nose internally, precluding any aerial communication, without producing a great difference in the remaining conditions. And it was absolutely necessary, after I had found that certain fixed salts, e. g. bicarbonate of soda, sulphate of copper, nitrate of silver, that emit no smells, not even if shaken with saliva, will nevertheless produce strong flavors when *tasted.* And though it might be difficult to tell *what* it is they impart to the air in the mouth, yet the air must be the vehicle or medium of their flavor, as it remains altogether unperceived by either closing the nose or by disconnecting the respective cavities, which I effected by the natural process, in which either the arcus glossopalatinus or the arcus pharyngopalatinus are so produced as to form a complete partition by the uninterrupted contact of the corresponding folds of the mucous membrane, the former running from the palate to the base of the tongue, the second to the back part of the fauces. The first I knew to be produced in using the blowpipe, and in the process of sucking, in either case respiration going on uninterruptedly between the nose and the larynx, while the air contained in the inflated cheeks is out of communication, and sustains a great pressure independently, by their very state of inflation or contraction the cheeks giving proof of a complete interception. The posterior arch or pharyngopalatinus is obtained by arresting the organs in the situation they take in deglutition, where, by the development of that partition, the morsel is prohibited from entering above the palate. Experimenting, therefore, under the mentioned conditions, with inflated cheeks, with intracted cheeks, or in a swallowing position, while the objects of taste were introduced into the mouth and moved by the tongue *the results in intercepting the inward communication proved the same*, invariably, as when the nose was *pinched.*

Taking, for example, a slice of pine-apple, it can be pressed between the tongue and the palate with cheeks inflated, and all its sweetness and acidity will come to full perception, but not what is called the *pine-apple taste*, which will, however, appear most beautifully the instant the inflation is relaxed inwardly.

When, in 1849, I exhibited the experiment on the person of an eminent physiologist, holding his long nose with a desperate grasp, to neutralize his involuntary efforts to disengage his "numb" taste, (he having always insensibly given way a little, while holding his nose himself,) he tried at first an explanation by the *strong squeezing* of the ramifications of the nervus ethmoidalis of the trigeminate nerve, supplying also the point of the nose. Indeed, in *this* instance, the explanation was not so much ludicrous from the scientific as from the dramatic point of view.

Some time afterwards, in order to disconnect the mentioned cavities, an experiment was tried in Vienna on some self-sacrificing individual, who had to stand on his head in a pail, water being filled in up to the level of the palate, leaving the oral cavity free and isolated. What were the results arrived at, I could not learn.

Pungency, acridity, burning and cooling sensations, and acerbity, astringency, or shrivelling, being doubtless cutaneous sensations, the *proper* sensations of the oral organ are shown by experiment to be summed up under the generic names of *sour*, *sweet*, *salt*, and *bitter*, four classes perhaps of incoördinate values, but which languages generally have discriminated by names; and it is curious enough, that none seems to exhibit any classification of flavors or nose-sensations proper, — classification being here still more needed, on account of the vast scope, the nice discriminations, and the important action of the organ.

The bitter taste is almost exclusively confined to the hindmost part of the tongue, and probably corresponds to the disposition of the nervus glossopharyngeus, while the nervus lingualis and other branches of the trigeminate nerve supply the rest, and the hypoglossus the muscles of the tongue, while the chorda tympani and the ganglion maxillare also deserve the greatest attention in this respect. I will here introduce the observation of a sympathetic task-sensation: touching aphthous ulcers on the mucous membrane of the jaw, especially with the point of the tongue, a slimy *taste of common salt* would, rather tardily, appear far backward on the margin of the tongue on the same side, the taste corresponding in intensity with the pain. The sour taste is strongest on the point of the tongue and along the margin to about the middle. Sweet and salt occupy the rest, rather promiscuously it would seem, and perhaps by transitions, which, however, cannot well be made out, unless one of the sensations can be suspended, or observed to be suspended.

Sour tastes belong to the tongue. Volatile inorganic acids are also perceived in the nose by their pungency and cooling, and by a slight smell similar to the (electric ?) smell of the spark produced by striking two pieces of quartz against one another. Organic acids, if they have at all a smell, have an aromatic one, as acetic acid. Many of the organic "acids" have most striking and strongly aromatic or fetid smells, although they would not be termed "acid" smells. The peculiar sensation of alcalescence is a *flavor* altogether. The following table will illustrate the exceedingly compound mass of sensations produced simultaneously on different organs by substances chemically pure. The cutaneous concomitant sensations are put in parentheses: —

Substances.	Specific Oral Sense.	Specific Nasal Sense.
Bicarbonas sodæ.	(Cooling), salt, bitter.	Alkaline, somewhat metallic or nauseous, *causing a great flow of saliva* (not if the flavor is suppressed).
Sulfas cupri.	(Astringent).	Disgustingly metallic.
Nitras argenti.	(Burning, astringent, smooth).	Metallic.
Acetas plumbi.	(Astringent), sweet.	Metallic, causing the "peculiarity" of its sweetness !
Bread.	Acidulous and salt.	Of bread.
Molasses.	(Somewhat pungent), sweet.	Of Molasses.
Divers sugars.	(± pungent), sweet.	Of impurities, or slightly burnt.
Acid. acetic.	(Pungent), sweetish, sour.	(Cooling), aromatic.
Acid. citric.	(Astringent), sour.	
Acid. muriat.	(Astr. pung.), sour.	(Cooling, pungent), slightly aromatic.
Acid. nitr.	Sour, saltish.	Slightly metallic.
Acid. nitr. fum.	(Astr.), sweet, then sour.	(Cooling, quickly pungent), electric, belong to a series of phosphoric, sulphureous, or arsenicose odors.
Acid. sulf. dil.	Sour, etc.	Faintly electric as above.
Spark of quartz.		(Exsiccating, dry, as dust of flour, etc.), "electric."

Substances.	Specific Oral Sense.	Specific Nasal Sense.
Aq. cinnam.	(Slightly astr.), sweet.	Of cinnamon.
Aq. cerasorum.	Bitter.	Mouldy.
Aq. camph.	Bitter.	Of camphor.
Aq. calcis.		Alkaline.
Alcohol.	(Pungent), sweet, acidulous.	Bracingly aromatic.
Aq. rosar.	(Slightly astr.)	Mouldy and of roses.
Aq. fœnicedi.	(Slightly astr.), sweet.	Similar to anise-seed, or roses.
Aq. menth. pip.	(Burning or cooling), sl. bitter.	Mouldy and aromatic.

The "series" of organic chemistry present as beautiful series of transitions in physical properties, and likewise of *odors*, as, for example, the series formic acid, acetic, metacetonic, butyric, valerianic, caproic, caprysic acid, together with their respective ethers and their semi-metallic similars. The same is observable in the hydro-carbons with their "burnt" flavors. Among the volatile chemical host I used to recognize old acquaintances from the woods and fields. The valerianic acid I had experienced before in valeriana and black walnut, the butyric in the rancid tastes of over-mature pine-apple, pawpaw, etc., the butyric ether in the strawberry, certain apples, the pine-apple, and smelled in the calycanthus, the alcohol in fallen prunes and other delicious fruit, when in process of coction by the rays of the sun. Similar analogies, corresponding to the series, may be observed in the ethereal oils, many of which are *sweet* to the tongue, as oil of cinnamon. Intensely sweet to the tongue, and perhaps also the palate, is air sucked into the mouth, that has come bubbling through an aqueous solution of chloroform. I have no doubt, that a nose and a tongue, properly erudite in the flavors and lingual tastes of substances chemically pure, might become very valuable re-agents on certain small quantities that permitted not of a chemical representation.

A thorough examination of substances under this point of view would, no doubt, by a cautious comparison of all relations, as far as accessible, show the essential conditions to olfactory and gustative perceptions proper, and lead to a comprehensive *classification* thereof. Besides, it would appear desirable to introduce two new and unequivocal terms to designate the two specific senses of the two organs mentioned, in order to avoid confusion, as it must be considered a very faulty practice to give current terms, as the necessity and convenience of social intercourse has created and understood them, a new and restricted scientific determination. "Taste" having hitherto been applied to the whole compass of multifarious and threefold specifically different sensations experienced in taking nourishment, and as this complex will ever recur henceforth as well as before, it would be exceedingly inept to apply that *term* to the class of the specific oral sensations. Less objectionable it would be to accept in this sense one of the synonymous words less frequently used, as, for example, "*savor*," while "*flavor*" might quite properly, as in common use, represent the specific nasal sense.

The nose, then, it appears, is the testing organ or warden, *not only for respiration, but more extensively for the function of digestion*, giving the chief and decided diagnose of the character of the food. This often had a practical bearing for me, and certainly has for many or most men, though mostly unconsciously. After having suffered much from the intermittent fevers of the West, all food that gave only a *dull smell*, or none at all, outwardly, as if the nose was benumbed, or that, if perceived, impressed the nose unpleasantly, causing a sort of constraint or headache in the lower part of the forehead, and at the same time producing a shadow of discomfort in the general or vital feelings, would also prove unwholesome, if taken notwithstanding; while such objects as could be perceived by a full and generous sensation of smell, at the same time exciting the appetite and general well-feeling, would prove easy of digestion, and wholesome.

I am hence inclined to think, that the general desire for food resides in the region of the stomach, as a sort of feeling of irritation and contraction in the pit of the stomach; that the desire for moisture and its savors resides in the mouth and the pharynx; and that the specific appetites reside in the upper nose, or, as it feels, in the lower part of the forehead.

The volatile and other substances that impart the peculiar flavors to each kind of victuals being generally present only in very minute quantities among the bulk of digestible matter to be assimilated, so that they could hardly be represented chemically, and being furthermore notoriously required to make food well-accepted, as, for example, a whole plate full of repugnantly insipid amylaceous victuals can be rendered appetitious by diffusion of two or three drops of white wine therein, so that the wine is not recognized as such, and still further, each article of food producing quite a *sui-generic* quality thereof with each different degree of heat, moisture, and admixtures, by the same process that renders it at all acceptable to the human system, it may well be inferred that they play an important part in the assimilating process, either as katalytics, or more probably by *stimulating the system* in each peculiar manner required (or perhaps both). This seems to be corroborated by the action of olfactory perceptions on the whole tonus of the system, being evidently very extensive, strong, and instantaneous; coffee-flavor, for example, on inhalation producing a feeling of hilarity and of strength in the pit of the stomach (solar plex ?); roses, an elating excitement; the odious smell of phallus, or of rotting mushrooms, a feeling of complete disgust and prostration on the stomach; metallic flavors, in the same locality, heaviness, nausea, or complete "*defaillance*"; other odors, the most innumerably diversified, sensations of purity, sweetness, pang, gloominess, delight, fervency, numbness, intoxication, narcotism, blandishment, strength, or aversion, etc., each in a single moment, as it were. Probably many of these feelings are compounds of the excitement caused by the olfactory perception itself, and of the divers *intestinal feelings induced by it*, and the inhalation into the lungs, as well as of their combined action on the *brain*; and I would here at the same time allude to the scent and instinct of food in animals. No doubt the *atmospheric smells and inhalations* here play an important part too. In fact, it might be said, that olfaction is a great stirrer or motor of the *vital feelings*, of gratification and disgust; hence the symbolic application of the idea of *taste* in all languages. Doubtless the eminently *ganglionic* structure of the olfactory nerve is in relation with its extensive action on the whole nervous feelings.

I have frequently observed in myself an astonishing degree of olfactory sensibility, simultaneous with an increased sensibility for light and still more for *color*, on awaking in the middle of the night, chiefly when disturbed by jolting or rolling, as will happen to travellers, — both, perhaps, owing to a common relation to the ganglionic system. I might give many more observations on similar subjects, but these will suffice to show how much might yet be elucidated in the physiology of the organs, by simply observing and comparing the facts as they offer themselves spontaneously.

2. THE WHALE-KILLER, OR THRESHER, MENTIONED BY TRADESCANT IN THE JOURNAL OF HIS VOYAGE TO RUSSIA, IN 1618. By DR. HAMEL, of the Imperial Academy of Sciences at St. Petersburg.

AT the opening of the present meeting of the American Association for the Advancement of Science, a communication was made by Lieutenant Maury, of the United States Navy, relating to the sea-animal which bears the name of Whale-Killer. The remarks offered on that occasion were founded on recent observations made by various American whalemen, but nothing was said to determine the place which the Whale-Killer occupies, and the name it bears, in the systematic classification of animals. It might not be uninteresting to recall to memory some former accounts of the Whale-Killer, to which I shall add an early one found by myself, in an unpublished manuscript.

It was reserved for me to discover at Oxford an anonymous autograph manuscript of a man, whose merits as an early collector of objects of natural history and as a zealous cultivator of plants in England, had been in a great measure forgotten, until I of late directed the public attention to them. I mean John Tradescant, the founder of the first museum of natural history and of various curiosities in England (at South Lambeth, London), which, as I have shown in a communication to the Imperial Academy of Sciences at St. Petersburg, was, along with his botanical garden, made over by a law decision of the Court of Chancery, contrary to his only son's last will, to Elias Ashmole, who had no right to it, but who presented it subsequently to the University of Oxford, after having received from it a diploma as Doctor. Tradescant's Museum has ever since been called The Ashmolean.

Ashmole may probably have destroyed all the writings of both the Tradescants, father and son, for none whatsoever have been found, except the one discovered by myself, and which neither he nor anybody else had recognized as being Tradescant's. It is the elder Tradescant's Journal, kept during a voyage which he made, in 1618, to Archangel in Russia, being in the suite of Sir Dudley Digges, sent there by King James the First.

When Tradescant returned from Archangel to England, he noted in his Journal, on the 14th of August (1618), being then near the coast of Lapland, and not far from Cape North, that there were seen many Whales, and that Mr. Nelson, one of the owners of the ship, the Diana of Newcastle, seeing one of the Whales pursued by a Thresher, called Tradescant on deck to witness the chase.

The Thresher mentioned by Tradescant is the same animal which is now commonly called Killer. It is *Delphinus Orca*, of late named *Orca gladiator*.

Sir Richard Hawkins, in the relation of his voyage through the Strait of Magalhaens to the South Sea, undertaken from England two hundred and sixty-one years ago, in 1593, mentioned the Thresher along with the Sword-fish fighting the Whale. He describes the battle as follows: "The Sword-fish placeth himselfe under the belly of the Whale, [how could Sir Richard see him there?] and the Thresher upon the Ryme [rim] of the water, and with his (broad and thicke) tayle thresheth upon the head of the Whale, till he force him to give way, which the Sword-fish perceiving, receiveth him upon his sword, and wounding him in the belly forceth him to mount up againe, and when in such manner they torment him, the fight is sometimes heard above three leagues distance, and I dare affirme, that *I have heard the blowes of the Thresher two leagues off, as the report of a peece of Ordnance*, the Whales roaring being heard much farther. It also happeneth sundry times, that a great part of the water of the Sea round about them, with the bloud of the Whale changeth his colour."

This fabulous account of battles given by two allied enemies to the monarch of the ocean has been, since the early part of the seventeenth century up to the present day, often repeated, with more or less detail, by voyagers and authors.

The first to do so was the Honorable George Percy, brother to the ninth Earl of Northumberland. He went, in 1606, with about one hundred persons, from England to Virginia, where, after the departure of the adventurous Captain John Smith, in 1609, he was for some time President of the first English settlement in the United States, founded in May, 1607, and called Jamestown. He was again, for a very short time, in 1611, Deputy-Governor, after Lord Delaware, the first Governor, had returned to England.

Percy writes: "Whilst we remayned at the Iland of Dominico, [where they arrived in February, 1607,] we saw a Whale chased by a Thresher and a Sword-fish. They fought for the space of two houres, we might see the Thresher with his flayle lay on the monstrous blowes which was strange to behold: in the end these two fishes brought the Whale to her end." Purchas, referring to this passage, says: "They had sight of the cheefe game which Nature yeeldeth, the fight of the Whale with the Sword-fish and Thresher."

Sir Richard's fable, although not well understood, was again, by William Strachy, inserted in his description of Sir Thomas Gates's and Sir George Somers's disastrous voyage from England to Virginia, begun in 1609. Strachy writes: "I forbeare to speake what a sort of Whales wee have seene hard aboard the showre followed sometime by the Sword-fish and the Thresher, the sport whereof was not unpleasant. The Sword-fish, with his sharpe and needle Finne, pricking him into the belly when hee would sinke and fall into the Sea; and when hee startled upward from his wounds, the Thresher with his large Fins (like Flayles) beating him above water."

Strachy was not aware, that by Hawkins's Sword-fish the *Xyphias gladius* was meant, of whose sword, "a bone, full of prickles on either side," he gives a description. Strachy evidently thought that a dolphin with an upright dorsal fin was meant, and indeed the *Delphinus Orca* has been called *Delphinus gladiator*, *Sword Grampus*, *Schwerdtfisch*, *Epée de Mer*, so that Strachy's Sword-fish and his Thresher would be one and the same animal. I need not add, that he was wrong in supposing the dorsal fin could serve as a pike.

The Reverend Edward Terry, who, in 1615, accompanied Sir Thomas Roe, when he went as Ambassador to the East Indies, mentions in the description of his voyage Hawkins's two attackers of the Whale. This was, in 1666, taken notice of in the first volume of the Philosophical Transactions of the Royal Society, in a "Relation of the Whale-fishery about the Bermudas and on the Coast of New England and New Netherland" in the following manner: "Concerning the Whale, which (two years previous) had stranded upon New England, it is not very improbable, but (that Fish having also more than one Enemy, whereof a small Fish called the Thresher, is one, who, by Mr. Terry's Relation in his East-Indian Voyage, with his nimbleness vexes him as much, as a Bee does a great Beast on the land; and a certain horny Fish another, who runs its horn into the Whale's belly) it may have been kill'd by the latter of these two, which kind of Fish is known sometimes to run its horn into Ships (perhaps taking them for Whales) and then snapping it asunder."

Richard Ligon, in his History of Barbadoes, where he went from England in 1647, mentions as the two mortal enemies of the Whale, "the Sword and Theshal fishes." The latter name is evidently a misprint for Thresher, but Dr. Grew, in his "Description of the Rarities belonging to the Royal Society of London and preserved at Gresham College," writes (1681), not discovering the error, Theshall, instead of Thresher.

The industrious, but unfortunate, John Lawson, having at the beginning of the eighteenth century come from England and travelled during eight years in North Carolina, mostly as surveyor of lands, says, in his account of that State: "Whales are very numerous on the coast of North Carolina, from which they make oil, bone, &c., to the great advantage of those inhabiting the sandbanks along the ocean, where the Whales come ashore, none being struck or killed with a harpoon in this place, as they are to the northward and elsewhere, all those fish being found dead on the shoar." After a short description of the four kinds of Whales, Lawson continues: "These fish [the Whales] seldom come ashoar with their tongues in their heads, the Thrasher (which is the Whale's mortal enemy, wheresoever he meets him) eating that out of his head as soon as he and the Sword-fish have killed him. The Thrashers [so Lawson writes, instead of Threshers] make good oil, but are seldom found. The Sword-fish is the other of the Whale's enemies, and joins with the Thrasher to destroy that Monster. After they have overcome him, they eat his tongue, as I said before, and the Whale drives ashoar."

The author of whom we first learn that by the New England whalemen the Thresher was called Killer, is the Honorable Paul Dudley.

The memory of this worthy gentleman is kept alive in Massachusetts more on account of the services which he rendered his Province as a superior guardian of the law, and because of the legacy which he (1750) appropriated to Harvard College, at Cambridge, for annual sermons to be preached there on matters referring to religion and the church. But it may not be superfluous to recall to memory his love for study, and his desire to render useful the various observations he had from time to time occasion to make on scientific and economical subjects.*

In 1719, he began to send communications to the Royal Society of London, which elected him a Fellow in 1721. An essay of his, "Upon the Natural History of Whales," was printed in the Philosophical Transactions of 1725. In this paper Dudley treats of those Whales which are taken by the New England whalemen. His information he had chiefly obtained from Mr. Atkins, from Mr. Greenleafe, and from Mr. Coffin, who had many years been engaged in the whalefishery. Besides the Spermaceti Whale, to which the New-Englanders had turned their attention about 1720, Dudley describes the Right or Whalebone Whale, the Scrag Whale, the Finback Whale, and the Bunch or Humpback Whale, which, he says, is distinguished from the Right Whale by having a bunch standing in the place where the dorsal fin is placed in the Finback. The pectoral fins are sometimes eighteen feet long.

Paul Dudley's account of the Whales was properly taken notice of only by three European cetologists, Klein, Anderson, and Brisson. Bonnaterre, Lacepede, and Desmarest did no more than just mention it. When Professor Eschricht of Copenhagen blames Ray for not having noticed Dudley's paper, he forgets that Ray died eighteen years before it was printed.

Eschricht has of late endeavored to prove that Dudley's Humpback is the Keporkak of the Greenlanders, the Fin Whale, so widely spread all over the ocean, and which had been, since Dudley's communication, described under a great many different names, without its being supposed that they might all be nothing else than Dudley's Humpback.

All the existing confusion might have been avoided, if proper attention had been paid by Artedy, and particularly by Linné, afterwards also by George Cuvier, to Dudley's chaste account of the Whale with a hump on his back, and with strikingly long pectoral fins. Instead of this, they put full confidence in the relation, contained in Martens's description of his voyage (1671), as barber on a whaling ship, from Hamburg to Spitzbergen, made up from his notes and narratives by Dr. Fogler, and printed in 1675. Sibbald's "Phalainologia," printed 1692, and again 1773, was also consulted.

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* I hardly need to mention here, that he was the grandson of Thomas Dudley, who in 1630 came over from England with John Winthrop, the Governor of the then lately formed Massachusetts Bay Company, he himself having been, just before the departure of the ship, appointed his Deputy. Born at Roxbury (3 Sept., 1675), Paul Dudley studied at Harvard College, and subsequently, whilst his father (Joseph) served in the Isle of Wight, he was for some time in London, chiefly to study the law. After his return, he filled high stations in that department. In 1718 he became Judge and in 1745 Chief Justice of the Supreme Court in Massachusetts, which appointment he held to the day of his death, the 25th of January, 1751.

Let us now turn to Dudley's account of the Whale-Killer. Speaking of the enemies of the Whale, he introduces this dolphin under the designation of a fish in the following manner: —

"Our whalemen [he means those of New England] have given this Fish (that preys upon the Whales and often kills the young ones) the name of Killers. These Killers are from twenty to thirty feet long, and have Teeth in both Jaws, that lock one within another. He has a Fin, near the Middle of his Back, four or five Feet long. They go in Company of Dozens, and set upon a young Whale and will bait him like so many Bull-dogs, some will lay hold of his Head, and bite and thresh him till the poor Creature, being thus heated, lolls out his Tongue, and then some of the Killers catch hold of his Lips, and if possible of his Tongue and after they have killed him, they chiefly feed upon the Tongue and Head, but when he begins to putrify they leave him. This Killer is without doubt the Orca, that Dr. Frangius describes in his Treatise of Animals. His words are these: 'Quando Orca insequitur Balænam, ipsa Balæna horribilem edit Mugitum, non aliter quam cum Taurus mordetur a Cane.' These Killers are of such invincible Strength, that when several Boats together have been towing a dead Whale, one of them has come and fastened his Teeth in her, and carried her away down to the Bottom in an Instant. And sometimes again they have bit out a Piece of Blubber of about two Foot square. The Killers are sometimes taken, and make good Oil, but have no Whale bone."

The question arises, Who is the Dr. Frangius whom Dudley mentions in his paper? — The name Frangius is a misprint for Franzius. Dudley quotes here from one of the writings of the learned German author, Wolfgang Frantze, D. D., who was born in 1564, and, having been many years Professor of Divinity at the University of Wittemberg, died there in 1628. In 1612, he had edited in that place a book, entitled "Historia Animalium Sacra in qua plerorumque Animalium præcipuæ Proprietates in Gratiam Studiosorum Theologiæ et Ministrorum Verbi ad Usum Εἰκονολογίκον breviter accomodantur." This little work was so much liked that it became necessary to print many editions. It was also rendered into English by N. W., and (1670) printed in London under the title of "The History of Brutes." Subsequently, John Cyprianus (born in 1642 at Rawitch in Great Poland, and deceased at Meissen in 1715) wrote a commentary and supplement to Frantze's book, which was first printed along with it at Dresden, in 1687. An edition in five volumes in quarto (Francfort and Leipzig) was printed in 1712.

Frantze, who was by no means a zoölogist, had been collecting materials for this his work to direct the clergy, and, still more, theological students, whilst occupying themselves with natural history, to the "omnipotence, wisdom, and goodness of the Creator." He repeats this sentence over and over again. The (eighth) Earl of Bridgewater, in his legacy (of 1825), which produced the well-known English (Bridgewater) treatises, used the very same words. Speaking of the Whale, Frantze says: "Sentiunt Theologi, quod sub aquis plura et mirabiliora extent vestigia potentiæ, sapientiæ et bonitatis Dei, quam inter animalia terrestria et aerea." Our divine knew only Whales, dolphins, fishes, and other animals of considerable size in the waters. The world of living beings there, which Ehrenberg, Bailey, and others have since, by means of the microscope, brought before our eyes, remained hidden from him. He says, however: "Dei sapientia et potentia in parvis creaturis magis est conspicua quam in vastis belluis."

Dudley was not aware that Frantze had taken the passage about the Orca, which he (Dudley) quoted, from Rondelet's work, "Libri (XVIII.) de Piscibus Marinis in quibus veræ Piscium Effigies expressæ sunt," and where (in Book XVI. Chapter XIII. page 483) we have a description, and also a representation, although a very poor one, of an Orca. This standard book was edited at Lyons, it is now just three hundred years since, viz. 1554. The second part, bearing the title, "Universæ Aquatilium Historiæ Pars Altera, cum veris ipsorum Imaginibus," is dated 1555.

Rondelet says: "Orca . . . dentes habet valde latos in acutum desinentes, serratos, quibus balænam persequitur, quam cum mordet, veluti mugitum tauri a canibus comprehensi cogit edere." He adds:

"Quam ob causam, nautæ qui piscatus causa in novum orbem navigant, barbaros rogant, vel si liceat, imperant, ne orcas lædant, aut venentur, quoniam orcarum opera balænas, phocas, aliasque beluas capiunt. Orcæ enim truculentis dentibus beluas alias impetentes maris gurgites cogunt relinquere et ad litora confugere, quas illic sagittis telisque aliis interficiunt."

I must mention, that an account of the Thresher, which occurs in a work published in 1737, in Dublin, and which has been quoted as an authority on the subject by Pallas, in his *Zoögraphia Rosso-asiatica*, and, in Russia, by myself, in my *Tradescant*, is, as I have found since my coming to America, a mere transcript of John Lawson's above-quoted notice, printed in 1709. I allude to Dr. John Brickell's book, entitled, "The Natural History of North Carolina, with an Account of the Trade, Manners and Customs of the Christian and Indian Inhabitants," &c.

Lawson, being Surveyor-General of North Carolina, was, in 1711, requested by Christopher Graffenried, a Swiss from the Canton of Bern, to whom the title of Baron had been given, and who was the leader of the colony of immigrants from Switzerland and from the Palatinate, settled at the confluence of the Neuse and the Trent at New Bern, to go with him up the river Neuse to explore the lands, when they were both seized by the Indians, and Lawson was cruelly put to death. Brickell, in compiling his book, takes the liberty to copy Lawson, often word for word, without acknowledging it. He does so even in the preface. The reader is induced to believe that Brickell had himself viewed what he merely transcribes. His account of the Thresher is altogether taken from Lawson's.*

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* Lawson had (1709) said: "I not only survey'd the Sea Coast and those Parts which are already inhabited by the Christians, but likewise view'd a spatious Tract of Land, lying betwixt the Inhabitants and the Ledges of Mountains, from whence our noblest Rivers have their Rise, running towards the Ocean, where they water as pleasant a Country as any in Europe..... Great Part of this pleasant and healthful Country is inhabited by none but Savages, who covet a Christian Neighbourhood for the Advantage of Trade.... But not to amuse my Readers any longer with the Encomium of Carolina, I refer 'em to my Journal and other more particular Description of that Country and its Inhabitants which they will find after the Natural History thereof, in which I have been very exact, and for Methods sake rang'd each Species under its distinct and proper Head."

Brickell, in his Preface, writes (1737): "I have viewed not only most part of

J. Hector Saint John de Crevecœur, a Frenchman, who was a long time in America, in his account of the whale-fishery in Nantucket, gives (1787) the Killer also his other English name, Thrasher, so that if it was wanted, we have a proof that Thrasher, or, as it was originally written, Thresher, and Killer mean the same Dolphin. He says: "The second enemy [of the whale-fishers] is much more terrible and irresistible [than the shark, of which he had spoken before]; it is the Killer, sometimes called the Thrasher," &c. In another place this author says: "The Killer or Thrasher is about thirty feet long; they often kill the other Whales, with which they are at perpetual war."

It would be useless to quote all the various authors who describe attacks made upon the Whale by the two allied animals, the Sword-fish and the Thresher. A few days ago, immediately after Lieutenant Maury had made his communication, such a battle, modelled after Sir Richard Hawkins's account, composed at the end of the sixteenth century, appeared in a Washington paper, the National Intelligencer. It was headed "The Killer Whale," instead of "The Whale-Killer." We are here again informed, that whilst the killers attack the whale at the surface of the water, the sword-fishes are (seen ?) waiting and following

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the Lands Inhabited by the Christians, but likewise vast spacious Tracts lying between them and the Mountains, from whence our noblest Rivers have their rise, running for several hundreds of Miles towards the Ocean, while they water and adorn as pleasant and fertil a Country as any in Europe, the greatest part whereof is only inhabited by Savage Indians, who covet a Christian Neighbourhood for the advantage of Trade. But not to amuse the Reader any longer with Encomiums on Carolina, I refer them to my Description of that Country, and it's Inhabitants, which they will find in the following Natural History, in which I have been very exact; and for Methods sake, have ranged each Species of Animals, Vegetables, &c. under distinct and proper Heads."

Speaking of the Thrasher, Brickell copies the above-quoted passage of Lawson's book thus: "These Fish (the Whales) are never found dead or floating to the Shoar with their Tongues in their Heads, for it is the Opinion of many in these parts, that the Thrashers and Sword-Fish (which are mortal Enemies to the Whales where ever they meet them) eat the Tongue out of their Head, as soon as they have killed him. . . . . The Thrashers make good Oil, but are seldom found in these Parts. The Sword-Fish, or Saw-Fish . . are mortal Enemies to the Whale, about which they gather in great Numbers, and when they kill him, only eat out his Tongue, as is supposed, and then the Whale floats to Shoar. They likewise joyn with the Thrashers to kill them."

far below, and when the whale dives, the latter are (seen ?) "darting at him with the velocity of lightning," and perforating with their swords his belly. This attack (seen through the water and the whale) sends the whale again to the surface; the described manœuvres are then twice repeated. After that the boldest of the killers is said to seize the whale near the throat, and to tear away skin and fat. The whale now opens his mouth, whereupon the killers seize his tongue and tear it out; they are reported to do the same with his eyes. Sharks, that had been (seen ?) along with the sword-fishes below, now come, together with these, to the surface, and the combined army of killers, sword-fishes, and sharks begin to feed on the expired whale.

I have yet to quote a modification of these fights, where the sword-fish has, not an orca, but a fox, for his ally. The fish of the shark kind, which, on account of its long caudal fin, resembling a tail, had already in olden times the name of Fox (*Vulpes*, *Alopias*), goes now mostly by the designation of *Carcharias Vulpes;* it is also called *Alopias* (and *Squalus*) *Vulpes.*

Dr. William Borlasse, Rector of Ludgvan (near Penzance), in Cornwall, says in his Natural History of that County, printed in 1758: "We call this Shark the Thresher from the motion of its long fox-like tail, with which it strikes or threshes its larger and less agile enemy, the Grampus, whenever it reaches to the surface of the water to breathe." He adds: "This engagement lasts several hours, as I have been informed by an eyewitness." This witness was the Rev. Mr. Dyer, Vicar of St. Clare, north of Liskeard.

Grampus was one of the names given to the Killer, the Orca; and if this was meant here by Dyer and Borlasse, then we have a battle between a fish and a dolphin, each called Thresher.

The name of Thresher for the Sea-Fox has been, after Borlasse, adopted by many authors. In Europe it was first done by Pennant in his British Zoölogy, where he makes use of some of the words of Borlasse, without, however, naming him. In America this Thresher was noticed and figured (1805) in the Medical Repository, conducted by Dr. Mitchell and Dr. Miller, a specimen of it having been found in 1803 on the coast of Long Island.

Captain Hugh Crow, of Liverpool, mentions (1830), in his account of a voyage from thence to Memel, a battle seen by him, near the Hebrides, given to a whale. To make out a description of it *à la*

Hawkins, modern books on ichthyology seem to have been consulted in order to learn what sort of an animal Hawkins's Thresher was, and finding this name placed against the little Fox Shark, this long-tailed fish was, of course, made the whipper-on for the sword-fish, *Xyphias Gladius*, (seen ?) waiting underneath for the whale. But considering the slightness of the tail, observed in the drawing, Captain Crow appears to have thought it just to reduce the noise produced by the blows dealt out with it upon the whale's back, which Hawkins had compared to the report of a piece of ordnance, to that of "the report of muskets fired at a distance."

Tradescant did not meddle with Sir Richard Hawkins's fable, which was in print before he went to Russia; he simply states that he was called upon deck to see a whale chased by a thresher.

The Thresher or Killer, the Orca, which had been called in England Grampus, likely from Grand Purpois, and in France Marsouin, evidently from the German Meerschwein, has had various other names; but as the same were occasionally given to other dolphins, it is hardly worth while to mention them. North Caper it seems to have been called first in the neighborhood of Cape North. The Greenlanders call the Orca, Ardluk. So says Eschricht. Before him it had been stated that the Tikagulik of the Greenlanders was the Whale-killer of the New-Englanders.

Our Russian fishermen along the coast of Lapland, in which locality Tradescant mentioned the Thresher, have named the Killer Kossatka, which word is made from Kossa, meaning a scythe, because the characteristic long dorsal fin of this dolphin has much the shape of a scythe.

One of our most experienced and intelligent fishermen in those regions, a citizen of Mezen, Aleksei Okladnikoff, has given me details in regard to the Kossatka. He informed me, that very often they chase whales into the long bay, called Motoffsky, which is formed by the coast of Lapland and the southwestern shore of the peninsula Ruebatchy, so called from *Ruebak*, a fisher, *rueba* meaning a fish. The torturers and killers of the whales are here so active, that the shore of Motoffsky Bay and its indentations (the small bays Kutowsky and Titowsky, both the latter names most likely corrupted from Kitowsky, *Kit* (κῆτος), meaning a whale) very often exhibit skeletons of whales.

It is remarkable, that the Rev. Dr. Scoresby, who has formerly been so much among the whales, never knew this their enemy. He says, referring seemingly to the stereotyped fable of Sir Richard Hawkins: "The Sword-fish and the Thresher (if such an animal there be) may probably be among the enemies of the Whales, but I have never witnessed their combat."

Tradescant, of whom I may, on some other occasion, communicate more details, came to Russia in 1618, as I have stated, with Sir Dudley Digges, whose father, Thomas, and his grandfather, Leonard, were well-known geometers, and who was himself one of the most enterprising members of the Muscovy Company.

On the 26th of February next (1855) it will be three hundred years since this Muscovy Company (now called the Russia Company) was formed in London, in consequence of the very kind reception given in 1554 by Tsar Iwan Wassiljewctch, in Moscow, to Richard Chancellor, who, being in one of the three ships of Sir Hugh Willoughby's expedition, sent forth in 1553, by the advice and at the instigation of Sebastian Cabot, from England to the Northeast, (when Willoughby, with two of the ships, entered the bay Nokooyeff, on the coast of Russian Lapland, where he, with all his company, sixty-five persons, was frozen during the winter of 1553-1554,) had, by accident, got into the White Sea. On the 24th of August (1553) he landed before the village Nenoksa, a little to the west of the monastery of St. Nicholas and the islands of the Dwina Delta.

On the westernmost of these islands, named by the English, from the great abundance of wild roses, Rose Island,* their Russian seaport establishment was founded, whilst in Moscow they had their chief commercial house in Kitai Gorod on the Warwarka. Here lived for a great many years Francis Cherry, whose name, in 1604, was given to the island, solitarily situated in the ocean between the northernmost extremity of the European continent and Spitzbergen, because an expedition, sent by him the year previous to the north had found that this island (the Bear Island of the Dutch) might become important to trade, on account of its immense number of sea-horses or morses (in the Russian, ***morsh***). This discovery not only brought other English ships there, but it indicated to the members of the Muscovy Company the way to Spitzbergen, where, in 1610, a high mountain was named Muscovy Company's Mount, and where, in 1611, through Sir Thomas Smith's enterprise, the whale-fishery was begun by members of this company. Already in 1575 it had been projected by the English to introduce that branch of industry near the coast of Russian Lapland, where Tradescant, in 1618, noticed the whales and the thresher. William Burrough, then the Muscovy Company's factor in Russia, was consulted on the subject. He had been, in 1553, on board of Chancellor's ship, along with his elder brother Stephen, who gave the North Cape this name.

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* Tradescant brought roses from thence to his garden in London. In the catalogue of the plants, printed by his son, this rose is called Rosa Moscovita; Moscovic Rose.

Not to mention here the voyage projected by Sebastian Cabot, then Governor of the Muscovy Company, and performed by the said Burrough in 1556, with a view to try whether a northeast passage might not be found, nor a similar trial made in 1580, nor even John Davis's northwest voyages in 1585, 1586, and 1587, provoked chiefly by members of that company, with William Sanderson at the head, I wish to remind, that it was Sir Dudley Digges who, along with some others of the Muscovy Company, in 1610, sent Henry Hudson on that famous expedition in which he discovered the strait, and the great bay, or rather sea, which for ever will bear his name, as will likewise the river first navigated by him in 1609. For the said reason, Digges's name was given by Hudson to an island (which he had taken for a cape) near the entrance to that bay, where he soon after, along with his son, miserably perished.

Sir Dudley Digges continued, more than anybody else at that time, to encourage the geographical researches in the Northwest. He wrote to that effect a treatise, which was, without his name, printed twice, in 1611 and 1612, and he was the chief promoter of Thomas Button's expedition, with Nelson, in 1612. In the instructions for Button's voyage, signed by Henry, the Prince of Wales, Digges's Island in Hudson's Bay is mentioned as perhaps offering a convenient place of rendezvous for the ships.

It was again Sir Dudley Digges who, along with Sir Thomas Smith and other members of the Muscovy Company, in 1616, despatched Robert Bileth, his pilot being William Baffin. This expedition reached, as is well known, to an unsurpassed height in that vast oceanic expansion between land to the north, which should have been called Bileth's Sea, but received the name of the subordinate Baffin, as a bay. Here a cape, high up in the so-called bay, was named after Sir Dudley Digges, and Sir Thomas Smith, who was long Governor of the Muscovy Company, and had been himself, in 1604–1605, as first Ambassador from King James the First, in Moscow, — after whom three different localities in the northern part of Spitzbergen had already been called, — got his name now, in 1616, deservedly placed on the map of America of all others the nearest to the North Pole. Thus distinguished it remained two hundred and thirty-seven years, but recently there have been motives to write names above that of Sir Thomas Smith.

The very first Governor of the Muscovy Company, at its foundation in 1555, was Sebastian Cabot, through whose zealous endeavors in recommending the first expedition from England to the Northeast was brought about that friendly and mutually useful intercourse between Great Britain and Russia, which has lasted exactly three centuries, and to whom, together with his father, Great Britain owes all her possessions in North America, but who has no monument in London; — nay, who has been allowed to die in England, it is not known when and under what circumstances; nor can it be found out where the mortal remains of the immortal Cabot were deposited.

THE following papers were read, but no copy of them has been furnished for publication: —

3. ON THE WHALE. By LIEUTENANT M. F. MAURY, U. S. N.

4. ON THE DEVELOPMENT OF MONOCOTYLEDONOUS STEMS. By J. DARBY, of Georgia.

5. SKETCH OF THE GENERAL ZOÖLOGICAL STRUCTURE OF THE REGION OF COUNTRY IN CONNECTION WITH THE UNITED STATES AND MEXICAN BOUNDARY LINE. By DR. C. C. PARRY, of Davenport, Iowa.

6. LIFE IN ITS PHYSICAL ASPECTS. By CHARLES GIRARD, of Washington, D. C.

III. GEOLOGY AND PALEONTOLOGY.

1. The Cretaceous Basin of the Rio Bravo del Norte. By Arthur Schott, of Washington, D. C.

The Rio Bravo, from its head down to the Gulf of Mexico, is sending its waters through two distinct basins. These are an upper and a lower one. Geographically spoken, the former might be called the mountain, and the lower the gulf basin. In order, however, to conform the names to the following geological sketch, they may be called the carboniferous and the cretaceous.

Whilst engaged in running a surveying line along this river, commencing at the mouth of the Puercos, and extending to the mouth of the San Juan in the vicinity of the Mexican town of Camargo, and thence travelling down the valley to the mouth of the river, I, as an eyewitness, had the opportunity to collect some remarks about the various features of the surrounding country along that line. An account of what I saw there may, at least, offer some addition to the knowledge of this section of the Rio Bravo, which I intend to call its lower or cretaceous basin.

This is bordered towards the southwest and west by the easternmost range of the Mexican Cordilleras, the so-called Sierra Madre, the mountains of Monterey, Monclova, Santa Rosa, and other places in the State of Cohahuila. This mountain range crosses the valley of the Rio Bravo, above the mouth of the Puercos, in the great bend of the former, thus separating the cretaceous and the carboniferous basins, forming from there upwards the east border of the upper basin.

In the vicinity of the above-mentioned town of Santa Rosa, a sort of geological interruption has taken place, and, according to my observations, (if I conclude right,) it is at this part where another range of a similar nature is branching out in a northeastern direction. This range, consisting also of volcanic and eruptive strata, is less visible in the neighborhood of Santa Rosa, and on the west banks of the Rio Bravo, than on the Texan side, where it comes in sight in several places as a half-immersed crest of low hills and peaks, undeniably of volcanic origin. There are, however, indications of a perhaps more subterranean junction of this volcanic dike in Texas, with similar strata and marks of volcanic activity in times past, in the mountain range of Santa Rosa. Looking to the north from the road which leads from Eagle Pass on the Rio Bravo towards Santa Rosa, the common shape of cretaceous hills appears very much broken and disturbed. The same is the case in the valley of Elm Creek, some few miles above Eagle Pass. This valley is wide and flat, and also frequently intersected by irregular ridges and scattered groups of bluff-like hills. Besides this the presence of pretty extensive layers of lignite on both sides of Elm Creek seems to indicate here a disturbing catastrophe of past times. I therefore should not be surprised if a subsequent and more thorough examination of this section of the country, which circumstances prevented me from making, should really prove that all the localities here referred to are connected in a close relation to each other as the parts of one volcanic dike. Combining these suppositions with facts really observed, in the shape of basaltic hills, low peaks or detached strata of basaltic rocks in the vicinity of Santa Rosa in Mexico, and again near the head of Leona and Las Moras rivers, and on the western banks of Rio Trio, together with the accounts Dr. Roemer has given in his work, entitled "The Cretaceous Formations of Texas," I conclude that this dike branches out from the Mexican Sierra next Santa Rosa, and keeps on in a northeastern course, perhaps through all Texas, and possibly through the whole State of Arkansas.

The dike, however, is not to be considered as the real northern and northwestern line of the cretaceous basin of the Rio Bravo. It is only a part of such, the lowest of those terraces superseding each other even from the Gulf with its colluvial belt up through the country to the Guadalupe Mountains, which, on their part, are in the same and almost exact parallel direction with the dike. The latter, having protruded the uppermost portion of the cretaceous strata some distance below, where the lower strata of the same formation appear raised up, is to be considered as an outlier of the proper northern and northwestern limits of the lower basin of the Rio Bravo.

To the east this is bordered by the dividing ridges between the Rio Nueces and Bravo, whilst towards the southeast the basin dives under a colluvial belt, and finally into the salt waters of the Gulf.

Tracing the courses of all the various strata on a map, without regard to their respective age and nature, they would appear like so many water-lines or antediluvian tide-marks of the Gulf, the waves of which have been beating against those walls to the west and the north above described. These water-lines at the same time appear frequently and considerably interrupted by the current of the Rio Bravo and its tributaries. Numerous overthrows, cross-layers of younger strata, tertiary and colluvial banks, could be traced in the very vicinity of the river, and often far off in the midst of those vast arid prairies surrounding the different valleys, and also local basins between the main ridges.

To follow up the divisions and subdivisions of the different strata and geognostic changes in the structure of the country, it is the shortest and the best way to take the course of the river, which cuts through the strata almost generally at right angles. Thus the Rio Bravo offers a natural and very instructive cross-section.

Under the word "colluvial" I understand a region of alluvial and diluvial deposits alternately and irregularly arranged among themselves, just as the oscillatory action of the salt waters together with the pressing-down force of the fresh waters of the *terra firma* may have placed them. As they are only to be discerned by a closer and more carefully made examination, I prefer to bring them both together under the collective name "colluvial deposits." In want of fossil characteristics botany might offer some indications to that purpose, though such ought to be used under the circumstances only conditionally and carefully. The land of salt marshes, swamps, lagoons, and swimming beach, forming this belt spoken of, extends from the mouth of the river up to the Rancho Lomita, a distance of about seventeen miles. It is generally more open, showing only marshy thickets in the often very narrow bends of the river. The number of genera and species of plants, particularly of trees, being very much limited, shows there a series of real salt plants, thus proving the character of the seacoast. Long widely stretched ridges of calcareous clay intersect this country of lagoons. The whole surface shows a vast swimming level of old river-beds, bayous, and swamps, a twilight gift of creation.

In the bend above Rancho Lomita, a bare, arid, weather-washed ridge seems to form a kind of boundary between the maritime belt and the more fertile low lands of the Rio Bravo. Thence farther up the country becomes more and more timbered, whilst its lower parts, still a continuation of the maritime belt, abound in reed and canebrakes, and thickets of palmettos, willows, and other water-plants. In this section of the country the inhabitants raise sugar-cane and corn, of which the former especially prospers to all satisfaction. Here I saw palmettos of a more gorgeous growth than even on the banks of the Lower Mississippi. High lofty trees prevent here almost every far view, which would lead to some low ridges setting through these regions, though keeping off from the river for a long distance. The first bluff placed on a stratum of calcareous or cretaceous sandstone occurs next to a point called "Rockbluff" on the Texan side, and "Las Cuevas" on the Mexican side of the river. This locality is some distance above Reynosa, a Mexican town, which is built on a branch of a low cretaceous ridge, the lowest in the valley of the Rio Bravo. Here the banks of the river do not show any rock, and it is not until we reach Rockbluff that rocks of any description are visible. The stratum of cretaceous sandstone is about ten feet thick, but seems to increase on its course into the country.

Reynosa, about 150 miles from the coast, is to be considered as the lower limit of the cretaceous formation. From there upwards, almost the whole of the country is to be put under this head.

The strata of this cretaceous main show differences only in the lithologic character of their structure, that is from Reynosa up to the Las Moras. Here, nowhere are the general marks of the constitution of the upper cretaceous formation concealed. According to the forms of fossil remains, perhaps some subdivisions may be proposed. From Reynosa up to the San Juan, and from there to the vicinity of the Alcantras, next Mier, then farther up to the Salado River, all the strata of cretaceous green sand-rocks bear almost the same resemblance. If it is not the more frequent occurrence of extensive banks of fossil oysters of the largest size, no comparative peculiarity is connected with this section. This oyster-type seems to be of a quite later age, and, with its abounding presence at various localities, the whole of this line is remarkable as an "oyster section."

The mentioned forms have been met with next Roma, 300 miles above the Gulf, at the Aroyo Hondo, and at the foot of the island "Las Ajuntas," in the vicinity of Mier, about 310 miles. They appear here in such a quantity that I named this place "Oysterpoint."

The same form abounds also in the "Shady Bluffs," some few miles above the former. Besides this, other forms of fossil remains occur, the characters of which are not yet determined. In a hill-ridge around Roma, lignite was met with, as I was told, though I did not find it myself in this locality. At Eagle Pass I saw in the hands of a resident there a piece of a fossil bone, taken out from a bluff near Camargo. I was also informed that such like abound there, together with numerous fossil shells. I collected myself a number of bivalved and univalved shells on the water's edge under a bluff range at the Island of the Patriarchs, about 325 miles above the Gulf. On the same locality fragments of conglomerate shoals of shells come to sight, as also higher up on the Salado River, between the mouth and the falls five miles from the Rio Bravo. Similar conglomerates mark the heights of the "Painted Hills," five miles below Laredo.

Another characteristic of the section between this place and Camargo is the frequently returning occurrence of certain Septarias. Among other forms I mention here nodules or balls formed of green sand near the Rancho Veleño, not far below the mouth of the Salado. These balls are of different size, from that of three-pounders up to that of the largest bombshells, from which I named our camp next to this place, "The Bombshell Rock."

The strata between the Salado River and Laredo do not differ much from the former. There prevails almost throughout the same greenish or yellowish sandstone, more or less soft and brittle, and inclining to wither out. From Laredo up to the Las Moras, about 570 miles above the Gulf, a similar green sand comes in sight, but it is intersected by strata of a blue limestone in various localities. This limestone is coarse-grained, sometimes very hard, and often intersected by shoals of shelly conglomerates. The fracture of it is shelly; the edges appear sometimes crystalline, transparent. The texture of this, and also of the green sand-rocks on this section of the line, is very often so much oolite-like, that I have taken for a long time this formation as truly oolitic. Magnetic iron ore, or nodules and boulders strongly impregnated with such, are copiously imbedded in most of those strata. This is especially the case from about fifteen miles above Laredo upwards to the Las Moras River. In some places banks and reefs are in and alongside the beach, where such composites are imbedded. Thus I mention, for instance, a table-rock island, full of large pieces of such contents, which, of a more durable nature, stand far out from the more weathered surface of the green sand-rocks. The appearance at a bird's-eye view of this table-rock was like a stone field of cast-iron roses, according to which I named this rock "Fairy's Rose-Garden." On several localities I used such like lithologic characteristics to designate localities for the sake of topography.

Besides the above-mentioned blue limestone, which is used at Laredo, Eagle Pass, and other places, as material for buildings, and also for lime-kilns, still other strata intersect the main strata of that greenish cretaceous sandstone. Especially is there a blue clay more or less hard, sometimes soft, like plastic clay. These strata are usually alternately intersected by other strata of still more earthy marls of different tints, and also by layers of loose sand containing more or less oxide of iron. On several places I found among these strata, three to four feet thick, shoals of oyster breccia of most solid consistency.

The Presidio Falls at the Islitas, as the Mexicans call the extensive rapids near the town of Presidio di San Juan el Bautista, are formed by a similar blue clay, which is the same some fifteen miles farther up at a place called the Devil's Pen. Besides these various argillaceous strata, layers of *débris* of analogous rocks intersect the green sand on this line.

Lignite coal also intersects these cretaceous strata in layers from one half to four feet thick. Remarkable for such is the neighborhood of Aroyo Sombrerodillo, the Lizard Hills, from ten to fifteen miles above Laredo; also the banks on the Mexican side of the Rio Bravo below the mouth of Aroyo Escondido, three miles below Eagle Pass; then the very vicinity of Eagle Pass, where there is on the opposite side the Mexican military colony, "Las Pietras Negras," undoubtedly named after these coal-layers. Higher up on the other side of Elm Creek Valley, the most extensive coal-layers prove a general similarity of the whole section of this line.

A German blacksmith, at Eagle Pass, opened at this place a small coal-mine for his own use. He tried also to trade with this article, sending it to San Antonio de Bexar. The expenses for transportation, however, and also the doubtful security of the roads on account of the Indians, brought this speculation to a sudden end.

Among other signs, the prints of dicotyledonous leaves in the coal specimens of Eagle Pass induced me to believe that this fossil, together with the matrix where it is imbedded, belong to a tertiary formation.

Here I have to mention still another interesting fossil, which I found in a similar cretaceous or tertiary *débris*, where usually on the Rio Bravo coal is met with. It is a sort of fossil resin, perhaps bitumen, which I discovered on the slope of the "White Bluffs," about twenty-five miles below Eagle Pass. I saw there only some few inconspicuous loose strings of it. As I was told by Dr. John Torrey, who examined it, this fossil is much like that which this gentleman had to examine some years ago in the British province of New Brunswick, to decide in the matter of a lawsuit. The presence of this curiosity in both these localities, north and south, may possibly prove some relation between those respective formations. The hill-slopes, bluffs, and ridges, on the line between Laredo and Elm Creek Valley, are also remarkable for the presence of large and very numerous pieces of fossil wood, apparently broken trunks of endogenous plants, of which a subsequent examination may classify some four or five different species. The largest boulders of this kind I discovered near Eagle Pass in a ravine, brought down by the waters. Thus I dug out one piece of one foot in diameter by two feet in length. Still larger specimens I saw, however, in the Mexican town of Santa Rosa, where they were brought in by the people, to serve in the streets as corner-stones, three to four feet long by one foot to eighteen inches in diameter. These specimens are found in the immediate vicinity to the east, on the foot of cretaceous or perhaps tertiary table ridges, which are covered there by considerably extensive basaltic dikes. The color of these pieces of endogenous wood is dark red, probably caused by an addition of oxide of iron, whilst the specimens on the Rio Bravo generally show a light gray or yellowish color.

From Las Moras up to Aroyo San Felipe, about 595 miles above the Gulf, apparently a lower link of the cretaceous system comes in sight. It is from this point up to the mouth of the Puercos, that I consider the strata more or less metamorphic. Between Las Moras and San Felipe a pale gray or whitish limestone lies on the surface. Its fracture is more earthy and more soft than that higher up in the valley. This limestone, according to the fossils found there, is also cretaceous. Throughout the valley and along the tributaries of the river, cretaceous green sand seems to continue, though sometimes interrupted. The above-mentioned cretaceous strata continue still farther up for some distance, but they finally cease to make room for that whitish earthy limestone, but not before undergoing themselves some lithologic change into the shape of variously tinted, though soft, limestone rock, which occurs in this zone of the cretaceous main, and also in the most different localities on this continent, and which, to the public, is already known under the often used terms, Painted Rocks, Painted Caves, Painted Walls or Bluffs, and so on.

The river-line between Las Moras and San Felipe, and also up towards the Puercos, shows such strata frequently.

The fossil Fauna of this section of the line is perhaps less numerously represented than that of the strata of the lower country, but conglomerate strata of shells formed almost exclusively of *Exogyra arietina*, Roem., occur here in various localities. At first I found them at the crossings of the Aroyas Pietras Pintas and San Felipe on the El Paso road. Afterwards I saw them farther down in the valley of the latter, forming there shoals of three to four inches thick. At last I met them near the Painted Caves, next the lowest crossing of the San Pedro, and then up between the Lepan Hills, scattered abroad like gravel. I found them in all these various localities mixed together, partly thoroughly siliceous, partly coated over by magnetic iron.

Various fossils of cretaceous character, found on the line between Las Moras and San Felipe, prove that even this metamorphic limestone belongs to the cretaceous system.

The hydro-topography of this section of the line offers perhaps a comparatively remarkable fact. The whole cretaceous basin from the mouth of the river up to the Las Moras is a waterless, arid region, intersected only by dry beds of *aroyos* and creeks. These have chiefly only a subterranean stream, forming sometimes a more or less shallow pond of a dirty greenish dull and brackish-tasting water. Elm Creek and the lower portion of Las Moras belong also to this class. It is different on the Mexican side of the valley. Here the cretaceous levels are backed by those sierras of igneous and metamorphic rocks, the mountains of Monterey, Monclova, and Santa Rosa, and upwards towards the old Fort San Vicente. It is for this reason that here several clear water-streams empty into the Rio Bravo, though not many.

Crossing the Elm Creek Valley on the Texan side, which is, according to my supposition, the ranging of the above-mentioned volcanic dike, some change is perceptible, which increases the higher the valley of the Rio Bravo is followed up. It is, however, not before the Las Moras, that clear water-streams are met with, and even the lower part of this stream is, as I stated before, of a more or less similar character with the streams of the country below; whilst the spring of Las Moras is partly sinking and trickling through those withered strata, where they under such contact change their water into the same greenish and brackish-tasting fluid that the streams of the lower country are carrying.

From Las Moras up, two similar creeks empty into the Rio Bravo, both of them decidedly brackish, and probably still gifts of the upper cretaceous strata. Finally, with the Aroyo Pietras Pintas commences a series of some beautiful limestone streams, of which there are four on a line of about forty miles in length. These are the Pietras Pintas, the Zoquete, the San Felipe, and Stampeide Creek. The length of these streams seems to have some relation to their situation on the line of the Rio Bravo. Thus the southernmost has the longest, the northernmost the shortest run. This fact proves perhaps an inclination of substrata from north to south, on which the mentioned streams rise, and which may correspond with the above-mentioned dike below the Las Moras. This stream has a course of about thirty miles in length, whilst Stampeide Creek scarcely runs two miles. The unchangeable abundance of water, and the bold constancy by which these creeks spring forth at once from their deeply cleft chasms, may lead to the conclusion that their origin is actually on strata less cleft and cracked than those covering the surface of the country they are watering. Their water is strongly impregnated with carbonate of lime. It incrusts in very little time everything coming into its sweep. I noticed on the Mexican side of the valley similar streams of about the same size and nature, named the San Diego and Rodriguez.

Leaving the valley of the San Felipe, and crossing that of Stampeide Creek, a mere crack in the higher table-lands, the topography of the country shows considerable changes. Bold and massive walls of a dark gray and inside pale yellow tinted limestone constitute the country from here up to the mouth of the Puercos. This whole belt, fifty miles wide, is poor in fossils, though sometimes strata bearing an Isloniste-like character are met with. In an *aroyo* (ravine) about one mile and a half above the San Pedro, I found a single *Spatangus?* coated over by magnetic iron-ore.

The strata of the whole formation here may be more or less metamorphic, but they certainly are also of a varying structure. Some of them bear often the closest resemblance to Magnesian limestone.

Frequently large portions of the commonly vertical walls in the *aroyos* and river-beds are withering out, as if their inside had been formed of a softer material. A white dusty matter like chalk-marl is withering out from such rocks, the surface of which is rough (verrucose), as if these strata had been raised out of a heavy boiling pool. The stratification, though generally horizontal, shows sometimes local disturbances, by which the strata are brought into a synclinal or anticlinal position. The fracture of the rocks is sometimes grainy, somewhat crystalline, sometimes earthy and more dull. The walls of all those table-rock masses are commonly vertical, with their edges manyfold cracked and cleft. Often they are shaped as if erected by human hands, showing regular mason-work, walls, galleries, turrets, bastions, and other portions of fortifications.

The Flora in these regions exhibits also some difference from that of the lower country. Some inhabitants of the upper basin of the river are still present here, but disappear below the limits of this formation. There is, for instance, the *Fouquiera splendens*, which I only saw before in the upper valley of the river. Here it goes no farther down than the mouth of the San Pedro. *Lycopodium hygrometricum*, an inhabitant of the limestone along the San Pedro, Puercos, and upper Rio Bravo, does not go farther south than the left borders of the Aroyo San Felipe.

Different Filices (Ferns), having above the same localities, are confined here to the mouth of the San Pedro. *Anhalonium*, a Cactus, which was found first by myself near Fairy Spring, about ten to twelve miles above the San Pedro, and by Dr. C. C. Parry higher up in the valley, never was seen below that mentioned creek.

The latter is the only running stream between the San Pedro and Puercos. Though situated in another lithologic zone than the others more below, it seems to belong to the same hydrographic zone.

Its spring, boiling out from a deep clefty chasm in a solid mass of limestone, resembles very much one of the springs of the San Felipe. The length of its stream is still shorter even than that of Stampiede Creek, for it extends not half a mile. From here upwards the valley of the Rio Bravo turns gradually into an almost continuous *cañon* (mountain pass), formed on both sides of vertical walls from 120 to 150 feet high.

Those cretaceous oolite-like strata mentioned as pertaining to the upper strata of the chalk-formation appear between the San Pedro and Puercos only as mere offsets upon those table-lands. Here they form groups of irregularly gathered hills, representing there more a sort of antediluvial downs than regularly continued cretaceous ranges.

These offsets show, however, the same characteristics as below, and abound also in some few genera and species of fossils, especially in *Exogyra arietina.*

Having surveyed now the cretaceous basin of the Rio Bravo, following its geognostic structure stratum by stratum from the upper to the lower, there remains still another one, which at least ought to be mentioned. Not forming any geognostic belt or zone like the other, this formation is represented in these regions only by layers loosely distributed over the main, and imbedded in dells, clefts, or on the borders of valleys and ravines. According to the fossil remains discovered within, such layers are to be considered as tertiary deposits. There certainly is a greater number of them than I met with on the line above spoken of, and in some regions they may continue among each other for some distance. According to what actually was seen and examined, I mention here only three localities.

One, not far below the mouth of the Rio Puercos, shows in the narrow crack of a solid limestone wall a white mealy matrix (perhaps chalk-marl), full of fossils of various description.

According to Mr. Conrad, who examined them, some are new, as for instance *Caprina occidentalis* and *Nerinea Schottii*, Conr. The appearance of this crack, with its contents, leaves no doubt that these have been cast in from above. The crack towards its upper inlet becomes very narrow, showing only one half foot in width; below, it gradually becomes wider, until it reaches the bottom of the *aroyo*, on one border of which it is placed. The whole of it ends in a layer of about twenty-five feet in thickness by eighty feet in length.

Another locality where tertiary deposits have been formed is on the road between the Leona River and Eagle Pass, at the crossing of Aroyo Las Minas, situated in another cretaceous zone. Here *Avicula distans* and *Sanira Americana* have been determined also by Mr. Conrad.

It would be quite an addition to the physiographic knowledge of the cretaceous basin of the Rio Bravo to annex here some remarks concerning the Flora of the various regions, which constitute the main of the country to which this sketch refers. Time, however, has been until now too much limited to advance thus far in the examination of the botanical collections, which surely will offer an ample field for researches of this nature.

It may be, at another time, that the expectations of the public in this respect can be satisfied.

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2. NOTICE OF SOME SPONTANEOUS MOVEMENTS OCCASIONALLY OBSERVED IN THE SANDSTONE STRATA IN ONE OF THE QUARRIES AT PORTLAND, Ct. By PROFESSOR JOHN JOHNSTON, of Middletown, Ct.

IT will be the object of this paper to give a brief description of some spontaneous movements of the sandstone strata, observed in certain cases in this quarry, of which some notices have appeared in the papers, but no detailed description.

The Portland quarries, three in number, are situated on the east bank of the Connecticut River, some thirty or more miles from its mouth, and nearly opposite to the city of Middletown. They have supplied stone for building purposes to the inhabitants of the vicinity ever since the first settlement of the country, more than two centuries ago. Latterly there has been a large demand for the stone they furnish, from New York, Philadelphia, and other Atlantic cities; and every year immense quantities are sent down the Connecticut River, furnishing constant employment, during the season of navigation, for a large fleet of schooners, besides a large amount carried by other transient vessels. For some years past, the number of men employed in the direct business of quarrying the stone has been, during the summer, about fourteen hundred, and, in the winter, perhaps half as many as this.

I mention these facts in order to give some idea of the immense quantities of stone every year moved from the place; and, before proceeding to the topic we have specially in view, some general description of the quarries may be proper.

Tradition informs us that, at the time the country was settled, the sandstone strata, for a distance of a quarter of a mile or more, extended quite down to the river's bank, and in some places even overhung the stream; but they were long since removed, nearly as low as the surface of the water, many rods back from its edge. But though the strata made their appearance at the river's bank, it was only here, they being entirely concealed from view at a little distance back by a thick covering of "drift." Only a few rods from the river a burying-ground was early laid out and occupied; but the quarries have now nearly surrounded it, and soon it will be left as an elevated peak, being entirely inaccessible except by stairs from below.

I am not aware that the sandstone here possesses any peculiarities to distinguish it from the same rock in other parts of the Connecticut valley. Perhaps it may possess in a more eminent degree than in other places those properties which adapt it for use as a building material; but I imagine that in many other places, if the strata could once be exposed to view, the stone would be found of equal economical value. Here, as everywhere in the valley, the rock is beautifully stratified, the strata varying in thickness from a few inches only to many feet, and being separated from each other by beds of soft shale or marl. Some of the strata are composed entirely of fine silicious sands, but others often contain coarse gravel and pebbles of considerable size. These pebbles are of different sorts, but three kinds are more abundant than others; one composed of a peculiar ferruginous quartz, another of a flesh-colored, crystalline feldspar, and a third of a blue slate, very distinctly stratified. I have not been able to discover that any rock of characters similar to either of these now exists in this region. But the place of their origin of course may yet be found.

Here, as elsewhere in the Connecticut valley, the sandstone strata are slightly inclined from a horizontal position in an easterly, or rather a southeasterly, direction. This inclination is about twelve or fifteen degrees. No trap, so far as I know, makes its appearance at the surface nearer than three or four miles from these quarries, to occasion a disturbance of the strata.

In two of the quarries, as the excavation has been carried backward from the river, it has also been carried downward, so that an inclined plane has been formed for the removal of the stone; but in the other, — the one in which the movements to be described have been more particularly noticed, — the excavation has been made perpendicularly downward, at the distance of only a few rods from the water's edge. This excavation, which extends over about two acres, has now reached the depth, at the lowest part, of eighty feet or more from the level where the engine-house now stands, and probably one hundred and forty feet below the original level of the soil at that point. It is at the bottom of this deep cavity that the movements I allude to have been observed, though they were first noticed before it attained its present depth.

This movement consists simply in a gentle sliding of one stratum upon another, apparently in consequence of an immense lateral presence in certain directions, as will by and by appear. The movement, though taking place with enormous force, so as to crush portions of the rock before it, is slight, not exceeding three fourths of an inch. It takes place only in the general direction of north and south, and not at right angles to this, or in the direction of east and west.

The first instance observed occurred some two or three years ago, and occasioned some alarm among the workmen. The excavation had then reached the depth perhaps of one hundred and twenty feet from the original surface of the ground; and a beginning was to be made in the stratum then constituting the bottom of the cavity that had been formed. For this purpose the workmen were directed to cut with their picks a channel or groove, a foot or more wide, quite through the stratum, and some fifty or eighty feet in length. To facilitate the operation, the work was begun by the side of one of the natural joints or seams that occur at intervals, crossing the strata nearly at right angles; and as several workmen commenced upon it at the same time, all along the line it was made to sink with about the same rapidity, the loose fragments being thrown out as they were separated by the instrument. The stratum was found to be about six or seven feet in thickness, and it happened that the channel or groove was cut in an east and west direction. When the channel had been sunk to within about nine or twelve inches of the bottom of the stratum, suddenly the remaining stone at the bottom was crushed to fragments with a loud report by an enormous lateral pressure, and the men in alarm leaped from the excavation they had made. By examination, it was found that the walls of the groove or canal just made had approached each other about three fourths of an inch, the stratum through which they had cut having on one side slidden this distance in its ancient bed!

Since that time many similar instances have occurred; in fact, it always happens when a channel is thus excavated in a new stratum in an east and west direction, at or near the bottom of the quarry; but it is worthy of remark, that it does not occur, or only in a slight degree, when the channel is cut in the direction of north and south.

In their operations in this quarry the last year, the excavation was carried down in one part some twenty feet deeper than in the other part, but during the past winter a beginning was made in the part left by sinking a channel in the manner I have described. The stratum through which it was made happens to be at the place about thirteen feet thick, and the stone is very solid and of a fine grain. The channel, which is about one hundred and fifty feet in length, as is usual, was carried down uniformly through its whole length; and, just as was expected, when the thickness of the stone at the bottom had been reduced to about ten or twelve inches, suddenly the remaining stone was crushed by the enormous pressure, and the north wall of the channel advanced fully three fourths of an inch. As the strata are fully exposed to view at the eastern end of the channel that was cut, a good opportunity is afforded to determine both the fact of the movement and its extent, which are self-registered; and the evidence will probably remain there for some months to come, or until the stone is removed in the ordinary operations of the quarry. The slide of the stratum in this case was from north to south, for the reason, I suppose, that the channel was cut near the southern wall of the deep excavation.

These facts, I think, plainly show that the strata of sandstone at this place are not, at the present time, perfectly at ease in their ancient bed, but that, in some way, they have received a disposition to change slightly their position; and it becomes an interesting question to determine the cause, — a question, however, upon which I do not propose now to enter.

3. Brief Outline or General Description of a Remarkable Fossil, not known to be described, and by some supposed to be an Ichthyodorulite. By Professor William Hopkins, of Genesee College, Lima, N. Y.

The object of the following communication is twofold; namely, to give a brief outline or description of a very remarkable fossil, and to call the attention of paleontologists to the importance of being on the look-out for other and similar specimens.

Of the fossil under consideration, there are three specimens in my possession, — two of them found in the quarry near Auburn, Cayuga County, N. Y., and the other at Waterloo, Seneca County. The rock in which they were found is the corniferous lime, of which there are several quarries in the vicinity of Auburn. These quarries are of a variable thickness, and in some respects they differ from each other. In all of them, nodules of flint, or hornstone, abound in the upper surface, or capstone, of the quarry, and these are arranged in parallel layers, or ranges, from eight to ten in number. The color of the solid part of the rock is deep black. The quarry whence these specimens were obtained is about thirty feet thick, having nine layers. The fourth ascending layer is perhaps peculiar. In thickness it is about three feet, and numerous oblique divisions are apparent. The angle of elevation is about thirty-five degrees. The topmost layer shows eight ranges of nodules of flint, which are arranged in pairs, by which is meant, that there is more space between every two ranges than between each range. The unusual number of ranges, and the nodules of flint in this quarry, seem peculiar.

A general outline, or description of the fossil, or specimens, will now be given.

Figs. 1 and 2, representing specimens No. 1 and 2, will afford a tolerable representation of the fossil. Specimen No. 3 is not represented, though it is like No. 1, except that it is much larger. From the end or sectional view of No. 1, an idea of the upper and under surface in respect to shape, or form, may be conceived. Through the centre of the specimen there runs lengthwise an angular or carinated elevation, which in the drawing is represented by two broad lines, marked 1 and 2, — the one shaded white, and the other dark.

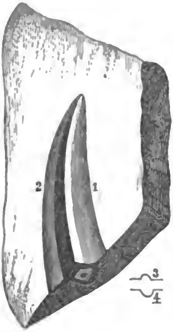

Fig. 1.

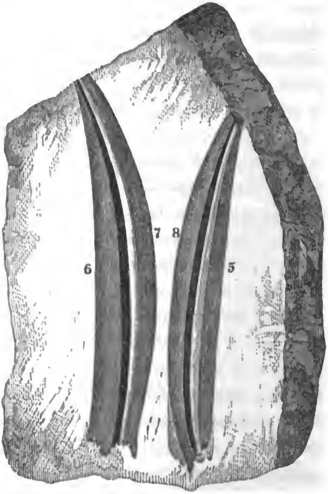

Fig. 2.

These lines also show the relative space, or surface, occupied by the carinated portion. Line No. 3 will perhaps assist in giving a better idea of the shape of the upper surface, while line No. 4 will show that of the under. The difference in form between the upper and under surface of the carinated portion is worthy of notice. The upper surface is quite angular, while the lower is elliptical, approaching a semicircular form, with the right side somewhat perpendicular, and the left more flattened, as if it were pushed up from beneath. This will be evident from an inspection of lines No. 3 and 4. Connected with this angular portion, there are also two flange-like projections from each side. These flanges are wedge-shaped, coming to an edge like the blade of a knife, and tapering towards the end. There is also a dissimilarity in these projections worthy of notice. The flange upon the right, in Fig. 1, is wider than that upon the left, and less uniform in width. The surfaces of these flanges are likewise marked with what appear to be three irregular lines, or rows of dots, upon each side of the carinated portion. These are more evident upon the narrower flange. There are also minute waving lines running lengthwise. The color and polish of the fossil are regarded also as remarkable. In color it resembles, as near as may be, highly polished whalebone or horn. It has, moreover, the different shadings which dark polished horn has, when presented to the light in different positions. In respect to finish, or polish, no art can excel it.

Fig. 2, representing specimen No. 2, is unlike No. 1 and 3. It is double, which perhaps may be regarded as a remarkable feature in the specimen, especially when taken in connection with what some have supposed to be its classification. From an inspection of the figure, it will be seen that the specimen is in pairs, rights and lefts, with so much symmetry as to justify the conclusion that it belonged to one animal, and that its relative position upon the animal was thus, which, if correct, might not justify the conclusion that it is an ichthyodorulite. It would be difficult to attach it to a fish in the position in which it is seen in the specimen, and still have it serve the purpose evidently designed from its formation. It may also be doubted about its being an ichthyodorulite, from its markings and the irregularity in its form.

There are only two conclusions to which the writer comes, and these are in respect to its use and the original composition of it. The one is, that it was an exceedingly powerful defensive weapon; the other, that it was once horn or bone. That it was a defensive weapon is evident from its form. Its coming to a point, having a sharp angular ridge terminating underneath in a circular shape, and its having knife-shaped flanges on its sides, are conclusive in respect to the design of it. That it was once horn, or bone, is apparent from its structure and appearance. The different layers, or growth, are evident to the naked eye, as is also the spinal cavity. Under a magnifying-glass these things are very distinct.

In respect to size, a word must answer. The length of No. 1, with the portion that was broken off, is about four and a half inches; the width of the basal portion about six eighths of an inch; the thickness of the carinated portion in one direction, three eighths of an inch; and in the other, two eighths.

The length of No. 2 is about five and a half inches, and the width about the same as in the preceding.

The length of specimen No. 3, not figured, is about seven and a half inches, and one eighth of an inch in breadth.

In respect to their classification no opinion is ventured. Great interest must necessarily attach itself to every appearance of the kind. When their true nature is ascertained, a new chapter, or new revelation, in respect to animal existence, will, it is believed, be brought to light.

THE following papers were read, or presented, but no copy of them has been furnished for publication: —

4. OBSERVATIONS UPON THE GEOLOGY OF THE MAUVAISES TERRES, NEBRASKA, WITH NOTICES OF THE GEOGRAPHICAL AND GEOLOGICAL RANGE OF SOME OF THE FOSSILS OF THAT REGION. By PROFESSOR JAMES HALL, of Albany.

5. REMARKS UPON A COLLECTION OF CRETACEOUS FOSSILS FROM NEBRASKA, AND THE ABSENCE OF SPECIES KNOWN IN THE SOUTHERN EXTENSION OF THE SAME FORMATION. By PROFESSOR JAMES HALL, of Albany.

6. REMARKS UPON THE RESULTS OF EXTENSIVE AND CONTINUED COLLECTIONS OF FOSSIL SPECIES FROM A PORTION OF THE SILURIAN ROCKS OF NEW YORK, SHOWING THE NUMBER OF SPECIES AND INDIVIDUALS OF EACH SPECIES OBTAINED FROM A LIMITED LOCALITY DURING A PERIOD OF TEN YEARS. By PROFESSOR JAMES HALL, of Albany.

7. ON THE REPRODUCTION OF SIMILAR TYPES OR REPRESENTATIVE SPECIES IN SUCCESSIVE GEOLOGICAL FORMATIONS. ILLUSTRATED BY A COLLECTION OF SPECIES OF THE BRACHIOPODA FROM THE NIAGARA AND LOWER HELDERBERG GROUPS OF THE PALÆOZOIC ROCKS OF THE UNITED STATES. By PROFESSOR JAMES HALL, of Albany.

8. GEOLOGY OF THE LEAD MINES OF WISCONSIN. By EDWARD DANIELS, Geologist to the State of Wisconsin.

9. ON THE AGE OF THE SO-CALLED NEW RED SANDSTONE OF THE UNITED STATES. BY PROFESSOR W. B. ROGERS, of Boston.

10. RED SANDSTONE OF THE CONNECTICUT RIVER VALLEY, AND THE PROOFS OF ITS OOLITIC OR LIASSIC AGE. By PROFESSOR JAMES HALL, of Albany.

11. Some Comparative Observations on the Carboniferous Strata of North America. By Professor H. D. Rogers, of Boston.

12. On some Phenomena of Cleavage Structure, and Metamorphism in Coal and other Strata. By Professor H. D. Rogers, of Boston.

13. On Phosphatic Organic Remains in the Palæozoic Rocks. By T. S. Hunt, of the Geological Survey of Canada.

14. On the Crystalline Limestone of North America. By T. S. Hunt, of the Geological Survey of Canada.

15. Remarks upon the Geological Formation of the Country along the Line of the Boundary Survey, based upon the Examination of Dr. Parry, made under the Order of Major Emory. By Professor James Hall, of Albany.

16. On the Chemical Composition and Metamorphoses of some Sedimentary Rocks. By T. S. Hunt, of the Geological Survey of Canada.

17. The Silurian and Devonian Systems, and the Nature of the Evidence for Drawing a Line of Separation between the two Systems in the United States. By Professor James Hall, of Albany.

18. On the Western Limits of the Cretaceous Formation on the Northern Continent of America, as evidenced by the Various Collections that have been made by Exploring Expeditions under the Direction of the Government of the United States. By Professor James Hall, of Albany.

19. On the Cleavage and other Effects Caused by Trap Dykes in the Middle Secondary Rock of Va. By Professor W. B. Rogers, of Boston.

20. Notice of a Peculiar Variety of Coal from Breckenridge County, Ky. By Professor B. Silliman, Jr., of New Haven.

21. On a Number of Mineral Species. By T. S. Hunt, of the Geological Survey of Canada.

C. PRACTICAL SCIENCE.

PATENTS.

1. On the Relations of the American Patent System to the Progress of Science. By Dr. L. D. Gale, of the United States Patent-Office.

New facts are contributions to knowledge, which, arranged and classified, constitute integral parts of science. The application of any one of these individual facts to a useful purpose constitutes the legitimate subject of letters patent. A patent, therefore, is an expression, or an illustration, of a law of science applied to some one of the useful arts. As the laws are few, but the facts numerous and constantly accumulating, it is easy to see that these may, and, if properly used, must, benefit science.

As the American Patent System, the subject of my remarks, has varied somewhat from time to time according to the character of the several acts of Congress on which it is based, and as the men of science of our country have not generally made themselves acquainted with this system, I propose to state, somewhat briefly, —

1. What the American patent system is.
2. What are its bearings on science, especially chemical science.

An American patent is a monopoly for fourteen years of the exclusive right to use, and sell to be used, the results of one's labors, whether they be mental or physical, or both.

Mere discovery does not entitle the discoverer to letters patent; but when the discoverer goes a step further, and points out the means he has devised to make the discovery subserve a useful purpose, the means or devices he has invented are the proper basis of a patent. Galvani could not have obtained a patent for discovering the galvanic fluid, nor could Volta for his battery, till he should show that it was applicable to some useful purpose by which the arts of life might be advanced.

The discovery of potassium and sodium could not have entitled the discoverer to a patent, although the process might have been so secured. *Laws or principles of nature are not subjects of patent.*

Newton could not have patented the law of gravitation, although he might have patented an instrument or machine that might be used in colleges and schools for illustrating the law, on the ground of a direct improvement in the arts of life.

The philosopher who discovered the law that carbonic acid is absorbed by water in proportion to the pressure which confines it, could not patent the discovery, because it was the mere discovery of what before existed. But the man who first applied this law to a useful purpose, in the apparatus called the soda-water fountain, was justly entitled to the exclusive monopoly of making and selling all the soda-water fountains in the United States for fourteen years.

From 1790 to 1836 the American Patent-Office was regarded as not specially favorable to the progress of the useful arts, and was often called a museum of rat-traps, churns, and quack medicines, and that from the fact that patents were granted irrespective of novelty. Since that time, however, the system of examining applications on the novelty and patentability of the invention, as provided in the act of 1836 and others subsequently passed, has done much to elevate the character of the institution, and to promote the useful arts, and, indirectly, science itself. The manner in which this is brought about I will proceed to state.

America, so far as I am informed, is, up to the present time, the only country in which the system of examinations has been carried out, or, till recently, even attempted; and is the only one where confidence is placed in a patent before it has been tested by the courts. So valuable indeed is patent property in the United States, that applicants are often induced to ask claims, the devices for which they admit are perfectly worthless, the only plea being, that, if they can get a patent, they can sell it for *so many thousand dollars.* England has observed the working of our system, which has so highly commended itself to her judgment, that she has in her recently published laws adopted in part the same system.

As a general rule, the men of science of our country have held themselves aloof from all connection with our patent system, and have not made themselves acquainted with the mutual advantages to themselves, to science, and to the country, which would naturally grow out of such acquaintance.

I am inclined to believe that this state of things has arisen from the impressions created in the early history of the Patent-Office, before the existing law was enacted, and when patents, so called, were granted for devices claimed, without questioning whether they were in existence before or not. This is still the system practised in France and in many other foreign countries.

In our own country the first patent act was passed in 1790, which stipulated that, upon the petition of any person or persons to the Secretary of State, the Secretary for the Department of War, and the Attorney-General of the United States, setting forth that he, she, or they have invented or discovered any useful art, manufacture, engine, machine, or device, or any improvement therein not before known or used, it shall be lawful to cause letters patent to be made out in the name of the United States, &c. This act, it will be perceived, does not look to or embrace chemistry in its provisions; but an amendatory act was passed in 1793, which embraced all the subjects covered by the existing laws.

From the organization of the office in 1790 to its reorganization under a Commissioner of Patents in 1836, a patent was granted in nearly all cases where it was asked, without any question as to novelty or utility. This system, although it accumulated a surplus fund of more than $ 300,000, did little for the advancement of the useful arts, and nothing for science. A vast number of nostrums and quack compounds were patented, and although the grant of patents for such compounds has almost entirely ceased for the last twenty years, we still hear of druggists annually preparing large quantities of so-called "Patent Medicines."

The celebrated Jackson and Morton patent for the use of ether is almost the only instance of a recent patent for a medicinal agent.*

About seven eighths of all the patents granted are for mechanical, and one eighth for chemical inventions. The former are illustrations and exemplifications of well-known and fixed physical laws, and mostly confined to mechanism or mechanical movements, and cannot therefore be expected to develop new laws or ultimate facts to any extent. It is otherwise with chemistry, where new laws and new facts are constantly being added to the original stock, and these, when recorded in the records of the Patent-Office, must have a favorable influence in extending the bounds of science, as I trust I shall be able to show.

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* It is proper to say, that Dr. Jackson did not avail himself of the benefit of the patent. He stated to me at the time of taking out the patent, or soon after, that his wish was to *secure the honor of discovery more than to obtain pecuniary reward.*

Formerly, much more than now, the chemical arts were in the hands of men who knew nothing of the principles of the science on which their art is founded. Hence it results, that, although new facts might be, and doubtless often were, discovered, yet, being in the hands of those who could not use them nor appreciate them, they were lost to the world, till some other accident more lucky than the last should reveal and record them.

In this manner the patent system, and more especially the American Patent System as now in operation, has had a happy and most powerful influence to increase our store of individual facts. The system of examination of all applications for patents makes it obligatory on the part of the applicant to describe his invention, so that any person acquainted with the art to which it is allied can use it. And although he may not be able to describe it in language, he is, in such case, compelled to resort to persons accustomed to do such business, and who write out the description which is deposited in the Patent-Office; and thus it becomes a permanent record, which is kept confidentially till the patent be granted or refused, and then, after a certain lapse of time, it is open to all who may wish to inspect it.

It was a long time after the Patent Law of 1836 went into operation, before any palpable results were manifested. Time was necessary to show the practical bearing of the system, which refuses to recognize as new any invention that could be found in any printed publication, or proved to have been in public use in any part of the world. The system does, therefore, make absolutely contributions to our stock of existing facts.

The question of the value of American patents granted under the system of examinations has been fully tested within the last few years, by the rapidly increasing sales and high prices of patented property in the United States. At the present time a patent that is not worth more than $ 5,000 is regarded as a very small matter, hardly worth devoting one's time to, while those of medium value range from $ 20,000 to $ 50,000, and those of the higher order from $ 100,000 to $ 500,000, and some few exceed $ 1,000,000. Under these circumstances, it is not strange that the cupidity of inventors should be stimulated to press into their service the facts of science that they may be converted into silver and gold.

The stimulus of anticipated gain by the monopoly on the one hand, and there being a real desideratum or necessity for the invention on the other, is in almost all cases enough to insure the production of it.

As an illustration of this principle, I would cite the modern seed-planter, and grain and grass harvester, and especially the latter. Up to the time of the reorganization of the Patent-Office under the act of 1836, there was not, as far as my knowledge extends, a grain or a grass harvester capable of practical operation in use either in this or in any other country.

On settling the great prairies of Illinois, Wisconsin, and other Western States, vast fields of wheat and other grain were planted, and when the time of harvest arrived more labor was called for than the man-power of the country could perform. Besides this, the great demand for laborers in harvest increased so much the price, that farmers could illy afford to pay it. These circumstances constituted the necessity of the case, and became, at length, the lawful mother of the present beautiful machine, called the Grain and Grass Harvester.

High price of labor in our country compels us to resort to machinery as a substitute for man-power; the result of which is now felt in every branch of mechanism, from the simplest implement to the most complex machine. So far as my knowledge extends, there is scarcely any one branch of industrial art in our country that has not been entirely changed, if not absolutely revolutionized, within the last twenty years; a large share of which is, without doubt, derived from the encouragement given by the Patent System, which, while it gives to the public a better article than that before in use, furnishes a cheaper one also; because the patentee, having a temporary monopoly of the manufacture and sale, can dispose of so many that a small profit secures a very large return. This state of the arts in the United States explains why it is that patents, and patent property, in this country, are so readily disposed of, and at such high rates. The certainty of monopolizing a good business, at such prices as the patentee chooses, necessarily secures a fortune to the possessor within the lifetime of the patent.

I have stated that chemical science is benefited by the American patent system, in consequence of the preservation of facts and results of experiment, the applications of which to the useful arts inventors seek to patent; and I am prepared to add, that many of the chemical arts are in advance of the science of the day. Facts are almost daily being stored up in the archives of the Patent-Office, which, if they were in the hands of men of science, would soon be posted up in its legers. There are many branches of the arts that have made considerable progress, that are hardly named in our scientific works.

Liquid India-Rubber.[*]

It has long been a desideratum to have India-rubber in the liquid state for the purposes of the manufacturer, and a great variety of substances have been used, mixed with the fresh juice as it runs from the tree; but none was perfectly successful till the experiments of Mr. H. L. Norris discovered that aqua ammonia will keep the juice liquid for an indefinite length of time. I have examined the liquid gum-elastic, and allowed it to be exposed till the ammonia evaporated and left the gum in the state of a stiff jelly. By adding ammonia it is again liquid. Alcohol precipitates it instantaneously; hence the facilities of the manufacturer in working the gum. The alcohol may be saved by redistilling, and the ammonia redistilled with it as spirit of ammonia, or the ammonia converted to sal ammoniac. I also learned a curious fact in my experiments, namely, that the rubber recently precipitated by alcohol and which is quite solid, if put into water is entirely soluble in it; but when once dissolved, it cannot be again recovered as rubber, but has undergone decomposition. If, however, instead of being put into water, it be allowed to dry as soon as precipitated, the rubber is preserved in its natural state.

Paper.

The extensive circulation of newspaper prints, and other light reading, has, for a considerable time, exhausted the ordinary source of paper from rags, even making a large allowance for those furnished from foreign countries, and our manufacturers are on the eve of a successful application of the American forest-trees to supply any amount of a fair quality of paper. The hickory, the cotton-wood poplar, the white pine, the cane-brake of the Southern States (*Arundinaria macrosperma*), have already yielded to the plastic art of the paper-manufacturer, and a fair article of paper is being manufactured of the several kinds of wood here mentioned, and by several different processes. In the main, the processes are as follows. The material is first cut into suitable lengths; it is then treated by dilute alkali, by acid, or by mechanical violence, to break down the woody fibre and remove by solution the gummy, resinous, and other soluble matters. These chemical agents are sometimes used in succession, and alternating with mechanical action. The chemical agents are next washed out, and others applied to complete the removal of foreign matter, and finally the resulting fibre (nearly pure cellulose) is bleached and washed ready for the engine.

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[*] The experiments announced, on the reading of this paper, a new mode of obtaining alcohol without distillation: I have repeated them, and the same was done by Professors Henry and Schaeffer at the Tower of the Smithsonian Institution, without being able to confirm them.

Although it is stated in the books of German, French, English, and other writers, that paper may be made from a great variety of vegetable substances, it does not appear that foreign manufacturers have succeeded in introducing a fair article of paper from this source, except perhaps in one or two instances. It was left for this country to create the necessity, and to supply the demand. It is by no means decided which of the numerous processes thus far tried will be found the best. Several of which I have examined the products are decidedly objectionable, from the fact that the ultimate fibres are torn and treated with mechanical violence in some instances, and in others weakened and made tender by the action of chemical agents, from being used in too concentrated a form. The most common mode of treating fibre is first to cut it into suitable lengths, and then expose it to the action of dilute alkali, in some cases accompanied with steam, and in others not. The alkaline is followed by an acid treatment, sometimes accompanied with mechanical agitation, and at others not. Sometimes the treatment is commenced with the acid, and followed by the alkaline action; but in all cases the last operation is bleaching and washing the fibre, there being left almost nothing else but pure cellulose, and this fibre is in a similar state with that of cotton or linen rags repeatedly washed and worn.

Illuminating Materials.

Another subject which is absorbing the attention of inventors at the present time is *illuminating materials.* The high price of lamp oil for many years past has directed attention to the available substitutes. Besides the steady progress that has been moving forward in the manufacture of illuminating-gas designed chiefly for cities and towns, the country at large has been supplied with various substitutes for sperm oil, such as purified stearic acid, cheap and impure oils redistilled, spirits of turpentine, various mixtures of this with alcohol, rosin distilled (usually called rosin oil), asphaltic and petroleum compounds, embracing benzole, naphtha, naphthaline, paranaphthaline, paraffine, bituminous coal distilled at a low red heat, too low to yield gas of any amount, lard oil, cotton-seed oil, and various others.

It is now about twenty-five years since the spirits of turpentine was first introduced into the mechanics' workshops and the shop-windows of the city of New York, as an illuminating material. About the same date Isaiah Jennings obtained a patent for mixing a certain quantity of alcohol with spirits of turpentine, constituting the first *burning-fluid* used in this country. From the introduction of the Jennings' burning-fluid to the present time, its use has annually increased, and the other materials above named have gradually fallen into the ranks of useful products for illuminating. As all the products of the various processes are more or less volatile and inflammable, so the use of them in the liquid state is attended with more or less risk; and as the loss of life from accidents by fire is very considerable, much time and ingenuity have been expended in adapting lamps to burn these fluids with safety. These have been a fruitful source of applications for patents, but vastly more so the processes and products of distillation of the various hydrocarbons, especially rosin, asphaltum, petroleum, coal-tar, &c., and even green wood of any of the resinous trees is claiming consideration as a suitable material for illuminating-gas. The gas used to illuminate the principal avenues and public buildings of Washington, until within the last two years, was from the oil manufactured from rosin.

At the present time attention is specially attracted to the various modes of distilling rosin in contact with various chemical agents, with the view of freeing the oil produced from the peculiar odor, or from color, or both, so that it may be used in the place of other oils, or with which to adulterate them. Although a large part of these processes are empirical, the results, by being recorded, often develop singular, and sometimes important facts, which, in the hands of a man of science, might lead to the development of a new law or principle. It will doubtless be remembered by most of the members of the Association, that some two years ago the newspapers of the day were filled with notices of astounding discoveries, said to have been made by a certain gentleman in New England, who announced that he had succeeded in converting the atmosphere into an illuminating material, so that our houses could be illuminated at a mere nominal cost. The sequel, however, showed that he had really invented nothing. But the chemist whom he employed to examine the subject did discover some practical points in a mode of burning benzole, the agent experimented on by him, and this method of burning the article was afterwards patented. It so happened, however, that when the patentee had obtained his patent, and looked about to find the article manufactured, he was informed that it could not be had or manufactured in the United States in quantities at any price that would warrant its use as an illuminating material. But the demand being made, it was soon met by the discovery, that when bituminous coal is heated to a low red heat, too low to evolve much gas, a large quantity of a semi-liquid hydrocarbon is distilled over, which on being treated in a certain way yields a large quantity of benzole. I mention this incident because it illustrates an important principle, that wherever a definite want is created in our community, the supply will soon follow.

Zinc-White.

White-lead, from remote ages, has been the chief material relied on as a suitable vehicle for paints, from the fact that it is partially soluble in oils. The fact that the artificial oxide of zinc is also soluble in oil was, I believe, first announced by Guiton Morveau, during the latter half of the last century, at which time it had no practical value in the arts, and he proposed to substitute it for white-lead in mixing paint; but the proposition met with no favor, and the subject was allowed to remain at rest, till it was again brought forward, in 1845, by MM. Leclere and Barruel, of Paris, who in that year obtained a French patent for a mode of manufacturing zinc-white of the ordinary spelter of commerce. An English and an American patent were also granted for the same invention.

It has been well known to chemists and mineralogists, for half a century at least, that there are immense beds of zinc ore in the States of New Jersey and Pennsylvania, which, from their mixture with foreign matter, have hitherto baffled all attempts to work them, although backed by the best scientific and mining experience.

But the anticipated demand for zinc-white paint, and the hope of at last finding some successful process of extracting it from the above ore, and securing to themselves the profit of their results, drew into the field a large number of competitors, several of whom have, it is believed, perfectly succeeded in obtaining nearly the whole of the zinc from mixtures of zinc oxide, silica, and iron, that have hitherto resisted all attempts at working them.

Thus it frequently happens, that the stimulus of hoped-for gains impels onward the inventor, and generally empirically, until his labors are crowned with success, and an abundant fortune is the happy result; or, on the other hand, he tugs on, till disappointment after disappointment, poverty, and an overwrought mind and body, compel him to yield, and he sinks, and is heard of no more. Such are the results of inventors who are not grounded in the principles of science. Had they been so, their experiments would have been so well conceived and executed, that an experiment, or a series of experiments, would have decided the whole question without a waste of time or money. But the facts themselves, when the invention proceeds so far as to apply for a patent, are permanently recorded in the Office, and thus become a resource from which science may draw for its future progress.

EXECUTIVE PROCEEDINGS

OF THE

WASHINGTON MEETING, 1854.

HISTORY OF THE MEETING.

The Eighth Meeting of the American Association for the Advancement of Science was held at Washington, D. C., commencing on Wednesday, April 26, and continuing through Wednesday, May 3. The Standing Committee held a meeting on the evening of May 3d, for the completion of unfinished business.

The number of names registered in the book of members in attendance on this meeting was one hundred and sixty-eight; and eighty-two new members were chosen. One hundred and ten papers were presented, most of which were read, but only a part have been printed. Some were thought unworthy of publication, and, in other cases, copies have not been furnished by their authors.

The Meetings of the Association were held in the Halls of the Smithsonian Institution.

The Annual Address was delivered by the retiring President, Professor Benjamin Peirce, on Friday evening, April 28th.

The Eulogy upon the late Sears C. Walker, Esquire, was given by Dr. B. A. Gould, Jr., on Saturday evening, April 29th.

No lengthened abstract of the proceedings, scientific and executive, of the Washington Meeting of the Association is necessary in this place, as they are contained in full in the papers and resolutions printed in this volume. The revisal of the constitution, and the codification of the various scattered resolutions, of a permanent character, passed since the organization of the Association, though the final action upon them has been postponed to another year, gave more than usual prominence to the ordinary business of this meeting, and somewhat encroached upon the scientific labors of the Association, which constitute its main object.

The officers elected for the next meeting are Professor JOHN TORREY, of New York, President; Professor WOLCOTT GIBBS, of New York, General Secretary; and Dr. A. L. ELWYN, of Philadelphia, Treasurer. The Permanent Secretary, Professor JOSEPH LOVERING, of Cambridge, retains his office for three years from August, 1854.

The Association voted to hold their next meeting at Providence, R. I., on Wednesday, the 16th of August, 1855, having received an invitation to visit that city from President WAYLAND, in behalf of the Faculty of Brown University.

During the meeting at Washington, the members of the Association were elegantly entertained, on different evenings, by FRANKLIN PIERCE, President of the United States; JEFFERSON DAVIS, Secretary of War; JAMES GUTHRIE, Secretary of the Treasury; and WILLIAM W. CORCORAN, Esq.

On Monday, May 1st, by the invitation of citizens of Washington, and under the auspices of the Local Committee, an excursion was made to Mount Vernon in the steamer Washington, and a generous entertainment was provided for the Association on the way. On their return the members had an opportunity to visit Fort Washington.

The Association was honored with invitations and attentions from various public corporations, associated bodies, and private citizens of Washington, Alexandria, and Georgetown; and liberal arrangements were made by the indefatigable Local Committee with many of the railroads, and with the proprietors of Willard's and the National Hotel, to transport and entertain the members of the Association at reduced rates. It was a subject of regret to the members of the Association in attendance that time did not allow them to avail themselves of all the abundant hospitality of Washington and its vicinity; but it will be found suitably acknowledged in the various resolutions of thanks which are hereto annexed.

RESOLUTIONS ADOPTED.

Resolved, That the publication of the Proceedings of the Cleveland Meeting be referred to the Standing Committee.

Resolved, That Professor J. P. KIRTLAND be instructed to withdraw the paper of Mr. BRAINARD from the published proceedings of the Cleveland Meeting of the Association.

Resolved, That the Standing Committee be authorized to act for the Association in any matters of business which may not have been completed at the time of the adjournment of the Association.

Resolved, That the Permanent Secretary be instructed to inform the Publishing Committee of the Cleveland Proceedings of the disappointment experienced by the members of the Association in not receiving the published Proceedings of the meeting held in that city in July, 1853, and to request this committee to expedite the publication of these Proceedings.

Resolved, That Professor ALEXIS CASWELL, Professor J. D. DANA, and Professor JOSEPH LOVERING act as a committee to appoint a Local Committee for the next meeting of the Association at Providence.

Resolved, That Dr. FRANKLIN BACHE and WILLIAM S. VAUX, Esq. be appointed a Committee to audit the accounts of the Treasurer.

Resolved, That the American Association desire to express their deep appreciation and admiration of the liberality and zeal for science of the Chilian government, as manifested in its establishment of an astronomical observatory, and in sending observers and instruments to Peru, to observe the solar eclipse of November 30, 1853; and to express their earnest desire that an example, so noble, may not be without its effect on other nations.

Resolved, That the thanks of this Association are due to the government of Shelby College and the owners of its telescope for their zeal in the interests of astronomy, and their liberality in intrusting their beautiful equatorial instrument to Dr. B. A. GOULD and Professor JOSEPH WINLOCK, that its capacities for advancing our astronomical knowledge might not remain unimproved.

Major W. H. EMORY having stated that he was able to present the results of his observations on moon-culminations at this early period, in consequence of the kindness of Professor G. B. AIRY, the Astronomer Royal of Great Britain, in furnishing Greenwich observations in advance of the date of their publication; and Professor BENJAMIN PEIRCE having remarked that the same courtesy had been repeatedly extended by Mr. AIRY to the Coast Survey and other Works of the government, on motion of Mr. PEIRCE it was

Resolved, That the American Association for the Advancement of Science offer their acknowledgments to Professor G. B. AIRY, Astronomer Royal of Great Britain, for the repeated instances of his kindness to men of science in the United States, in furnishing for the works of government and others in its charge the results of observations made under his direction at the Greenwich Observatory.

Resolved, That the Chairman of the Committee on the Eclipse be requested to superintend the publication of Professor STEPHEN ALEXANDER's paper on this subject, and to draw upon the Treasurer for the expense of the same.

Resolved, That the Association has listened with pleasure to the eloquent and instructive address of its late President, Professor BENJAMIN PEIRCE, and that its distinguished author be requested to furnish the Permanent Secretary with a copy for immediate publication.

Resolved, That the Permanent Secretary be directed to superintend the publication of this address, and to print twelve hundred and fifty copies.

Resolved, That one hundred and fifty of these printed copies be presented to the author of the address.

Resolved, That the Standing Committee be authorized to take order in regard to the publication of Dr. B. A. GOULD's Biographical Notice of the late Professor S. C. WALKER.

Resolved, That the Committee on the Constitution of the Association be authorized to report By-Laws at the next meeting of the Association.

Resolved, That the Standing Committee be requested to print the existing Constitution, and the Constitution as proposed to be amended, and to distribute copies among the members of the Association.

Resolved, That a Special Committee of twenty be appointed to arrange for the observation of the Solar Eclipse of May 26, 1854.

The names of this committee will be found with those of the other committees.

Resolved, That the Standing Committee have leave to take order in relation to the early publication of Professor STEPHEN ALEXANDER'S paper, suggesting the proper observations to be made on the occasion of the Solar Eclipse of May 26, 1854, and to add any other information which the Committee on the Eclipse may desire to distribute among the members of the Association.

Resolved, That the Permanent Secretary be allowed to put the Proceedings of the Washington Meeting to press two months after its adjournment, and that fifteen hundred copies of the volume be printed.

Resolved, That a Committee, composed of Professor BENJAMIN PEIRCE, Professor JOSEPH WINLOCK, Dr. ASA GRAY, and Professor LOUIS AGASSIZ, be appointed to advise with the Permanent Secretary in regard to the matter of publication after the separation of the Standing Committee.

Resolved, That Dr. J. L. LECONTE be requested to act as Treasurer, *pro tem.*, in the absence of Dr. A. L. ELWYN.

Resolved, That the Standing Committee be authorized to extend an invitation to the President of the United States, and the Members of his Cabinet, to Senators, and Members of the House of Representatives, to be present at the sessions of the Association.

Resolved, That the Standing Committees of the Sections be required to report to the General Meeting before two o'clock P. M. of each day the programmes of the several Sections for the following day.

Resolved, That the Standing Committee have authority to appoint a Special Committee to report at the next meeting of the Association in regard to uniform standards in weights, measures, and coinage.

VOTES OF THANKS.

Resolved, That the members of the American Association for the Advancement of Science have been deeply gratified by the manner of their reception at the Capital of the Union, and will long remember the public and private efforts made to render their visit profitable as well as pleasant.

Resolved, That the hearty thanks of the members of the Association be returned to the President and other officers of the Government, and the officers of the Army and Navy residing in the city, and the citizens of Washington, for the very hospitable, kind, and munificent arrangements made for their entertainment during the meeting which has just closed.

Resolved, That the thanks of the members of the Association be tendered to the Local Committee, for the arrangements made, in advance of and during the meeting, for the accommodation of the members, and for the very favorable opportunity of visiting Mount Vernon.

Resolved, That the thanks of this Association be tendered to the Regents and Secretary of the Smithsonian Institution, for the accommodations afforded for the meetings of the Association Sections and Committees.

Resolved, That the members of the Association offer their thanks to the City Corporation and citizens of Alexandria for their kind invitation for April 29th, and regret that the state of the weather prevented their attendance at the time appointed.

Resolved, That the thanks of the members of the Association be tendered to the President and Professors of Georgetown College, for their kind invitation to visit the College, and their offer of a collation, and the regret of the members that from the pressure of business the hospitality could not be accepted.

Resolved, That thanks be tendered to the Secretary of the Smithsonian Institution, the Officers of the Coast Survey, of the National Observatory, of the Mexican Boundary Survey, of the Crystal Palace in New York, and of the Washington City Young Men's Christian Association, for their invitations to visit the establishments under their charge.

Resolved, That the thanks of the Association be presented to the Trustees of the Medical College of Washington, for their polite invitation to use their rooms as a place of meeting.

Resolved, That the Association return thanks to the following railroad companies, who have offered the facilities of free return tickets to the members in attendance at the meeting: —

Baltimore and Susquehanna; Harrisburg to Pittsburg; Baltimore and Philadelphia; Erie Railroad, from New York to Dunkirk; Wilmington and Weldon; Baltimore and Norfolk Bay Line; Richmond, Fredericksburg, and Potomac; Baltimore and Ohio; Virginia Central Railroad; Wilmington and Manchester; Michigan Southern and Northern Railroad; Ohio and Pennsylvania (Pittsburg to Crestline); Cleveland and Pittsburg; Camden and Amboy.

Resolved, That the thanks of the Association be tendered to the Directors and Officers of the Alexandria and Orange Railroad, for their invitation to make an excursion on their road.

CORRESPONDENCE.

LETTER TO THE PRESIDENT OF THE REPUBLIC OF CHILI.

American Association for the Advancement of Science,
Washington, 20th May, 1854.

To the Minister of Foreign Affairs of the
Republic of Chili: —

I am directed by the American Association for the Advancement of Science to communicate to you, for presentation to his Excellency the President of the Republic of Chili, the inclosed resolution, which was adopted at the recent meeting of the Association in this city.

At the same time, I beg permission to add, that both the public institutions and men most eminent for scientific attainments in the United States have watched with lively interest the rapid progress of knowledge under the fostering patronage of your government. The brilliant example set by Chili has given an impulse to the pursuit of science elsewhere, whose good effects will be felt far beyond the limits of your own country or our own era, and many will yet be grateful for the influence on others of the liberal spirit manifested by your government, no less than for the benefits to science which it has directly conferred.

I have the honor to be,
Very respectfully, your obedient servant,
Joseph Lovering, *Permanent Secretary.*

Communicated by
J. M. Gilliss, *Local Secretary.*

American Association for the Advancement of Science,
Washington, 3d May, 1854.

The Standing Committee of the Association reported the following resolution, which was unanimously adopted: —

Resolved, That this Association desire to éxpress their deep appreciation and admiration of the liberality and zeal for science of the Chilian government, as manifested in its establishment of an astronomical observatory, and in sending observers and instruments to Peru, to observe the solar eclipse of November 30, 1853; and to express their earnest desire that an example, so noble, may not be without its effect upon other nations.

J. D. Dana, *President of the Am. Ass.*
Joseph Lovèring, *Permanent Secretary.*

REPLY.

Santiago, 29 de Julio de 1854.

He puesto en conocimiento de Su Excelencia el Presidente la comunicacion de Ustedes de fecha 20 de Mayo del presente año, i la resolucion acordada por la Comision Permanente de la Asociacion Americana para el progreso de las ciencias, en la cual sus Honorables Miembros se sirven expresar un voto de felicitacion a mi Gobierno por la proteccion que dispensa al cultivo de las ciencias.

En contestacion digo a Ustedes: que mi Gobierno agradece esa manifestacion acordada en su obsequio por los mas distinguidos sabios de la Union Americana, i que siempre será para él un motivo de peculiar satisfaccion contar con las simpatías de los hombres eminentes i propender con sus esfuerzos al adelanto de las ciencias.

Sírvanse Ustedes trasmitir esta respuesta a la Comision que representan i aceptar los sentimientos de la distinguida consideracion con que tengo el honor de ser de Ustedes.

Alento Seguro Servidor,

Antonio Varas.

A los Señores José Lovering i J. M. Gilliss, *Secretarios de la Asociacion Americana para el adelanto de las Ciencias.*

TRANSLATION OF THE REPLY.

Santiago, July 29th, 1854.

I have acquainted his Excellency, the President, with your communication of the twentieth of May last, and also with the Resolution adopted by the Standing Committee of the American Association for the Advancement of Science, in which its respected members are pleased to express their vote of thanks to my Governor for the protection he has extended to the cultivation of the sciences.

In reply allow me to say, that my Governor makes grateful acknowledgment of this manifestation made in his favor by the most distinguished scientists of the American Union, and that it will always be to him reason of peculiar satisfaction to count upon the sympathy of eminent men, and to contribute by his efforts to the promotion of science.

Be pleased to transmit this reply to the Standing Committee which you represent, and to accept the sentiments of distinguished consideration, with which I have the honor to be

Your constant servant,

ANTONIO VARAS.

To JOSEPH LOVERING and J. M. GILLISS, *Secretaries of the American Association for the Advancement of Science.*

LETTER FROM MR. AIRY, ASTRONOMER ROYAL OF ENGLAND.

Royal Observatory, Greenwich, August 21, 1854.

DEAR SIR: — On returning to this place after an absence of some length, I have had the gratification of receiving your letter of June 8, conveying the expression of the kind feelings of the American Association for the Advancement of Science towards myself, in reference to the occasional communication of astronomical results to Major Emory and other officers and men of science in the United States.

I fear that the Association have greatly overrated the debt to me for the slight services rendered. Even from a private individual such communications might be expected; and from an officer in my public position they may be fairly claimed as a right. But I have not the less pleasure in accepting the friendly acknowledgment of the Association; and I have peculiar gratification in thinking myself amicably connected with your great country, not simply for the social and political reasons which apply to all Englishmen, but also because from America have proceeded the last great improvements in astronomy and the sciences related to it.

I am, dear Sir, your very faithful servant,

G. B. AIRY.

JOSEPH LOVERING, Esq., etc., *Permanent Secretary of the American Association.*

INVITATION FROM BROWN UNIVERSITY.

Brown University, April 27, 1853.

TO THE PRESIDENT OF THE AMERICAN ASSOCIATION
FOR THE ADVANCEMENT OF SCIENCE: —

SIR, — I have great pleasure in presenting for the consideration of the Association the votes of the Faculty of Brown University herein inclosed. We earnestly desire that the Association will find it convenient to accept the invitation.

I have the honor to be, respectfully, Sir,
Your obedient servant,
F. WAYLAND, *President of B. Univ.*

Resolved, That, in view of the pleasures and advantages of personal intercourse with cultivators of science, and the importance of diffusing in this community the results of scientific research, we invite the American Association for the Advancement of Science, now about to convene in the city of Washington, to hold its next annual meeting in this city; and that, for the convenience of their meetings, we offer them the use of such of the halls of the University as may best suit their purposes.

Resolved, That the President of the University be requested to communicate to the President of the Association the foregoing resolution, with the assurance that it will afford us great pleasure to do what we can to promote the objects of the Association, and the comfort of the members during their session.

F. WAYLAND, *President of B. Univ.*

J. L. LINCOLN, *Secretary of the Faculty.*

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ERRATUM. — On page 304 it is said that the next meeting is at Providence, on Wednesday, August 16. It should be August 15.

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